SCIENTIFIC AMERICAN

No. SUPPLEMENT, 272

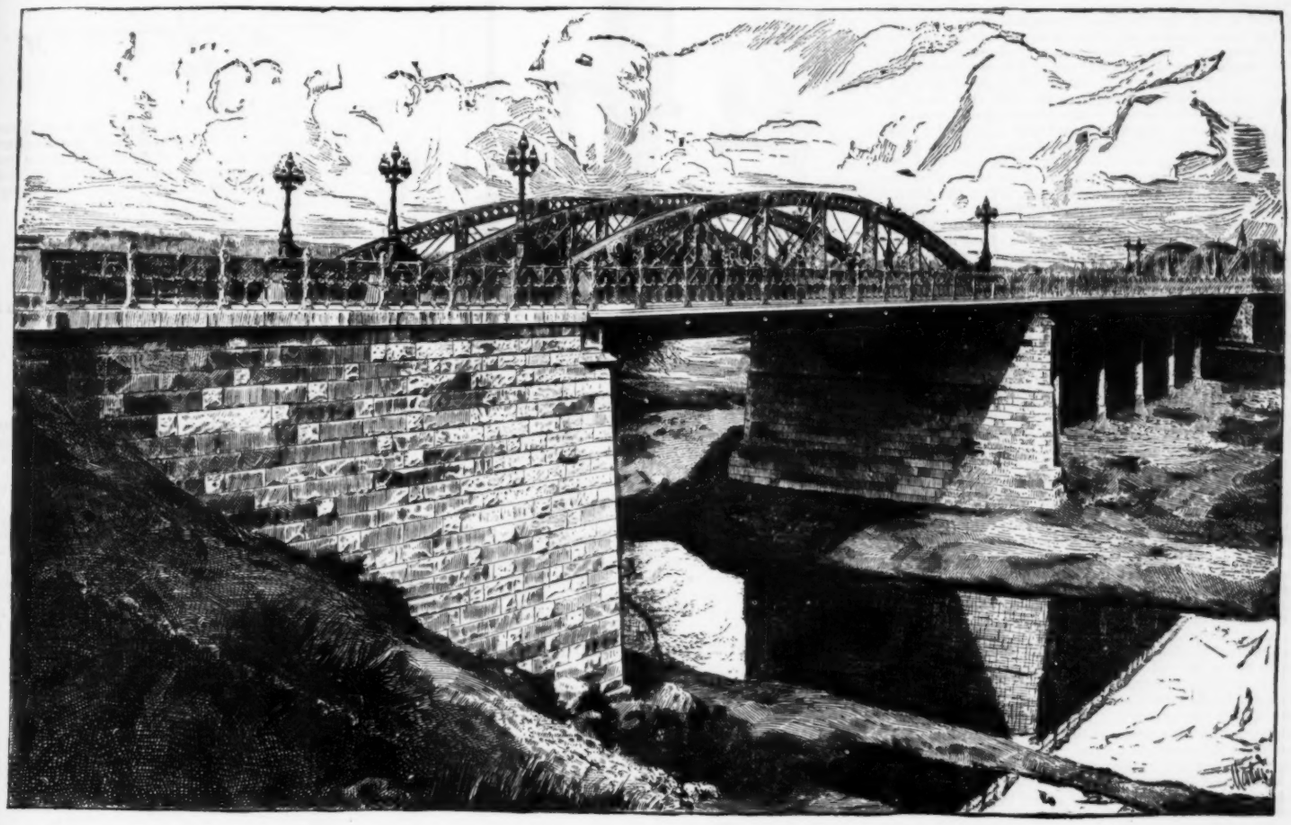
Scientific American Supplement, Vol. XI., No. 272.
Scientific American, established 1845.

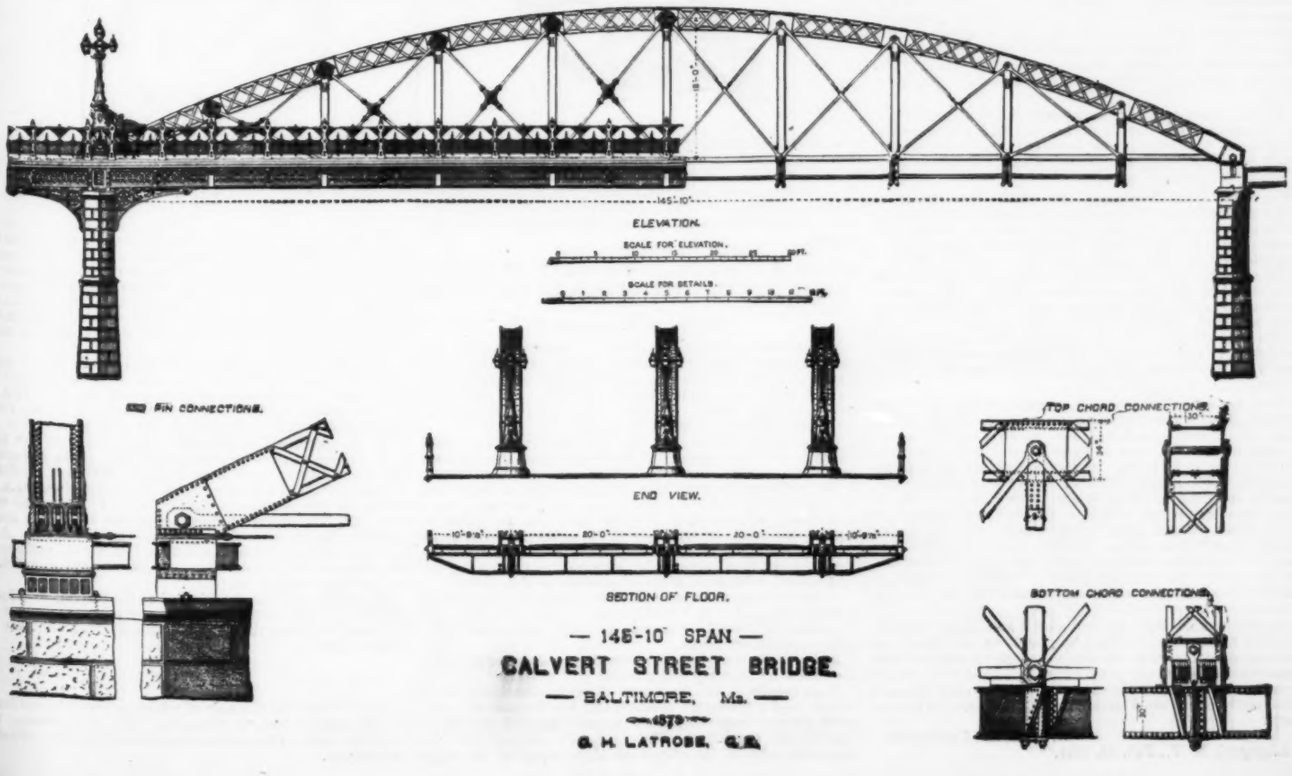
NEW YORK, MARCH 19, 1881.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

CALVERT STREET BRIDGE, BALTIMORE.

OUR engravings show this structure, which was built in Baltimore during the last year. It carries Calvert street, which is one of the principal streets of Baltimore, across Jones's Falls and the Northern Central Railroad. The latter is located in a ravine, along the stream, which somewhat curiously is called Jones's Falls, although there are no falls in it excepting those produced by mill dams. The grade of the street is considerably higher than the ravine, as is indicated by the engraving. The bridge has more decoration on it than is often found on iron structures of this kind.—*Railroad Gazette.*





DO STAYS STAY?

To the Editor of the Scientific American:

On page 98, in the issue of Feb. 12, of the SCIENTIFIC AMERICAN, is an article by Joshua Rose, wherein the distinguished engineer attempts to explain the explosion of the McCreery & Co. boiler, in New York City, on January 16.

While the theory therein set forth is an ingenious one, and to the casual reader may be satisfactory, there are no doubt many who, after reading it, will have a lingering conviction that the explanation itself needs explaining.

Parts of the article read as follows: "The stays, C, joined the cylinder of the dome 11 inches from the top (as marked in Fig. 2), and the crown sheet 7½ inches from the dome cylinder edge. Now, as the temperature of these stays increased they would expand and lengthen, exerting a pressure on the crown sheet in the direction of H. The steam pressure acting on the under surface of the crown sheet would also be in a direction to lift it; hence, as the pressure and temperature increased and decreased the crown sheet would lift and fall, bending it on A as a center of motion. The stays being at so great an angle would not be in a good position to resist this movement of the sheet. . . . Now, let it be noted that if a stay stands at an angle it will, under any increase of temperature above that at which it was riveted up, tend to push the two plates it connects apart (instead of holding them together), until the weakest plate has moved a certain amount. Thus, if the old stay, C, measures 15 inches, it will expand a certain amount per inch through a length of 15 inches; but the sheet of the dome will expand through a distance of its vertical height from the hole at I, or in this case 11 inches only; hence, to the amount that C would expand in 4 inches in length, it would push against the crown sheet, and not until the crown sheet endeavored to move still further would C begin to act as a stay. The same effect will be produced in proportion as the line of the stay varies from a right angle to the surface it is to stay, hence the stays, C or G, should be as near vertical as possible." Here the writer fails, it seems, to take into consideration the fact that there is an equal proportionate expansion of the upper horizontal plate, as well as the perpendicular plate of the dome and the stay.

Let us take for an illustration a right-angled triangle. Let the horizontal side be 12 inches, the perpendicular side 16 inches, and the hypothenuse 20 inches in length, and let these parts represent respectively a section of the crown sheet of dome, a section of the side of dome, and the stay. This figure will be identical with the figure which is represented in Fig. 2 of Mr. Rose's article, except that the length of the sides is a little greater, which difference, however, would make his conclusions still more apparent if they were true. Let us suppose now that there is a general expansion by heat takes place of an inch to the foot. The dimensions of the triangle will be: horizontal side 13 inches, perpendicular side 17⅓ inches, hypothenuse 21⅔ inches. Adding the squares of the two first dimensions, and we find the sum equal to the square of the third dimension. Hence, of course, the triangle is still right-angled and unaltered at the right angle, and yet each has suffered its natural expansion. If any further demonstration was necessary we might take a solid piece of iron, say 6x4x1 inch in dimensions. Bring this from a cool state to the temperature of superheated steam. Our triangle and that of Mr. Rose are represented in this piece of iron, the hypothenuse being found in a line running diagonally across and through the iron. This section of this piece of iron is subject to precisely the same influences as the stay in the boiler. If Mr. Rose's conclusions are correct the right angles will be forced into obtuse angles, and the sides will describe the arcs of a circle, a phenomenon which is not borne out by experience.

But granting for a moment that the explanation of Mr. Rose upon this point is correct, would, as he assumes, a longer stay help the matter any? So far as it acts more nearly in the direct line of the resistance to be overcome by its lessened angle it undoubtedly would; but so far as the lesser excess of expansion of the longer stay is concerned it would be difficult to see how it could. If there is any upward pressure of the expanding stay at a greater angle there must necessarily be some upward pressure at any angle, however slight. As there is no "loss of motion" here, the slightest upward tendency, even though the excess of expansion on the side sheet be the infinitesimal part of an inch, will make itself felt with almost equal force between two unyielding surfaces as when the discrepancy in expansion is greater.

Reductio ad absurdum, this supposed action of a stay can only be avoided by reducing the angle until it is lost in the side sheet of the dome. The inevitable conclusion of this would be that all stays except longitudinal ones are a source of weakness instead of strength.

The deduction at the close of the article that the crack which was pronounced to be not of recent origin was caused by the flanging of the sheet when made, probably, no one will dispute, but it is questionable whether, in the face of a hydrostatic test of 105 pounds, only five months before, a safety valve set at 60, and a daily working pressure of from 40 to 50, which probably sometimes ran up to 60 pounds, the theory that the boiler ruptured at 55 pounds will be generally accepted. Neither is it necessary to raise the theory, or rather no theory, of a "mysterious explosion," whereby through the "mighty magic" of some hitherto undiscovered agency a boiler is suddenly wrecked and no one knows why or how.

There are several ways that the explosion may have been brought about without hunting for a reason outside of the common and well settled conditions which we know can and do cause explosions. One of these is too little water, and another is too much steam. Safety valves are not always infallible, but sometimes stick or have their weights accidentally moved out. Feed water pipes and their connections sometimes spring a leak, letting the water slowly out of the boiler. Steam gauges, glass gauges, even try cocks, sometimes tell false tales. Boilers under defective sidewalks are liable to a sudden dash of cold water from above, or the slower but not less sure "constant dropping," causing unequal expansion and contraction as well as erosion.

Engineers, also, alas! are still mortal and liable to make blunders like the rest of mankind, and do not always remember such blunders distinctly enough to report them. However, in view of the meager data furnished so far, would it not be well to assign the explosion to some such common cause as the above until there is indubitable proof to the contrary?

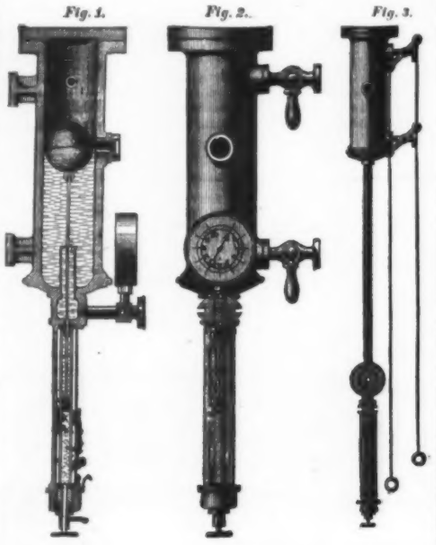
A. HERBERT CRAWFORD.

Liverpool, N. Y., Feb. 15, 1881.

IMPROVED WATER GAUGE FOR BOILERS.

THE improved glass water gauge shown in the annexed cuts is so constructed that it cannot be cracked or broken by hot water, an accident very common with glass gauge tubes. A cylinder is connected with the boiler in such a manner that the water therein will have precisely the same level as the water in the boiler. A float is contained in this cylinder, and has a wire attached to its underside. A pointer or like device, which moves up and down in a glass tube attached to the bottom of the cylinder, is attached to the lower end of the wire, and will thus rise and fall with the float. The water in the glass tube will always be cool, even if the

Fig. 1. *Fig. 2.* *Fig. 3.*



IMPROVED WATER GAUGE FOR BOILERS.

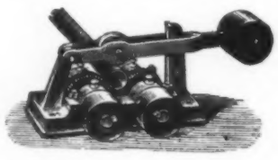
water in the cylinder is very hot, and the tube cannot be damaged by heat. It is incased in a suitable metal covering to protect from external injury.

The engineer can convince himself of the accuracy of his device at any moment.

The upper cock is closed, upon which the steam in the upper part of the cylinder will be condensed, and the float and the pointer will rise. After a short time the upper cock is opened again and the float and pointer will drop, and if the latter reaches the same level it had before, the apparatus is in order. The lower part of the cylinder is arranged to catch impurities so that they cannot pass into the glass tube.

DEVICE FOR CLEANING BOILER TUBES.

IN the annexed sketch a very simple but very effective device for removing scales from steam boiler tubes is shown. This apparatus is the invention of Mr. A. Meyer, of Aenzen, Germany, and will remove the scales from a tube in one or two minutes, whereas it would take more than ten times as long to remove the scales by manual labor. Two belt pulleys are mounted on two shafts, upon each of which a cog-wheel is mounted, and between these two cog-wheels the boiler



tube is placed, being pressed against the cog-wheels by a small wheel pivoted in a weighted lever. The wheel in the lever is slightly inclined towards the axis of the tube, and causes a spiral movement if the latter, which is thus gradually moved forward. The cog-wheel rotates from 500 to 600 times per minute, and scrapes the scales from the tubes very rapidly.

A NEWLY DISCOVERED PROPERTY OF THE ELLIPSE, AND ITS APPLICATION TO THE "OVAL CHUCK."

By FRANK M. LEAVITT.

IN studying to devise a machine for a certain purpose, the writer stumbled upon a mathematical property of the ellipse which he thinks to be not devoid of interest, on account of its apparently not having been noticed in any published treatment of the ellipse, and also on account of its practical utility for the purpose below mentioned.

In turning an ellipse on a lathe provided with an "oval chuck," the disadvantage is met with that at only four points, viz., at the extremities of the major and minor axes, is the surface of the work in a position normal to the cutting tool, half of the time the upper face of the latter being at an acute angle with the tangent to the ellipse at the point of cutting, whereby the tendency is to spring the tool away from its work, and during the remainder of the time, the angle formed being obtuse, the pressure tends to spring the tool inward, with the liability of making too deep a cut.

A similar state of things exists when a blank of sheet metal is being trimmed in an "oval chuck," and it is in this case so serious an obstacle as to make it totally impossible to obtain a satisfactory result.

It was while studying for a remedy in this latter case that the following property was noticed:

If an indefinite line be drawn through the center of an ellipse, and if through a point on this line situated at a constant distance from the point of intersection with the ellipse (and outside the latter) a line be drawn making with the transverse axis of the ellipse an angle equal to the angle made with the same by the line drawn through the center, the portion of the prolongation of the line so drawn included between its point of intersection with the line through the center of the ellipse and a normal to the latter at its intersected point, will be of a constant length.

To make this clear by Fig. 1, draw any line through the center, as O D. At the constant distance, A D = C, draw through the point, D, a line, M D E, making the angle, D M O = D O M. From the point, A, draw the normal, A E. Then will the distance, E D, included between the normal and the line, O D, be constant for all positions, in the same ellipse, of the line, O D.

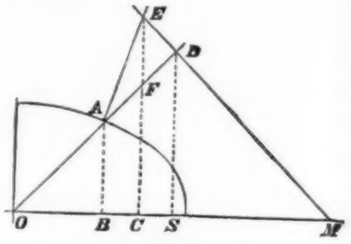


Fig. 1.

This may be proved as follows:

Let r be the *radius vector*, O A, of the point of intersection, A, α the angle, A O B = E M O, x' and y' the co-ordinates of the point, A, the origin of co-ordinates being at the center of the ellipse. φ and y, the co-ordinates of the point, E, and a and b respectively the semi-transverse and semi-conjugate diameters of the ellipse. Let E D = R. We have:

$$x, = \cos.\ \alpha\ (r + c - R)$$
$$y, = \sin.\ \alpha\ (r + c + R)$$

also

$$x' = r \cos.\ \alpha$$
$$y' = r \sin.\ \alpha$$

which values, substituted in the equation of a normal to the ellipse,

$$(y - y')\ b^2\ x' = (x - x')\ a^2\ y'$$

gives after reducing

$$R = \frac{a^2 - b^2}{a^2 + b^2} c.$$

Hence R is constant for each ellipse, and its value depends on the values of the major and minor axes of the same, multiplied by the constant, c, and varies from R = 0 when $a = b$, or in the case of a circle, to R = c when $b = 0$.

The manner in which this principle may be applied to a lathe furnished with an "oval chuck," is illustrated by the skeleton drawing Fig. 2, which is lettered similarly to Fig. 1.

The slotted arm, A E, swings freely on the pivot, A, which is fixed to the carriage of the lathe, and to this arm is fastened the cutting tool, t, by a proper tool post not shown in the sketch. The tool must be so placed that its cutting edge is in line with the center of motion, A, of the arm, A E, and of course enough in front of the pivot on which the arm vibrates as not to allow the latter to interfere with the work.

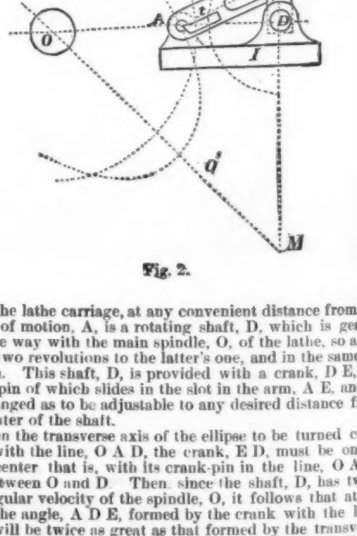


Fig. 2.

On the lathe carriage, at any convenient distance from the center of motion, A, is a rotating shaft, D, which is geared in some way with the main spindle, O, of the lathe, so as to make two revolutions to the latter's one, and in the same direction. This shaft, D, is provided with a crank, D E, the crank-pin of which slides in the slot in the arm, A E, and is so arranged as to be adjustable to any desired distance from the center of the shaft.

When the transverse axis of the ellipse to be turned coincides with the line, O A D, the crank, E D, must be on its inner center that is, with its crank-pin in the line, O A D, and between O and D. Then, since the shaft, D, has twice the angular velocity of the spindle, O, it follows that at all times the angle, A D E, formed by the crank with the line, O D, will be twice as great as that formed by the transverse axis with the line, O D, or A D E = 2 D O M, a condition required in our proposition. Also, the fixed distance, A D, represents the constant, c, the crank, D E, being the constant, R. It therefore follows, from the conditions above set forth, that in all positions of the chuck the arm, A E, and consequently the tool, t, will be in a normal position to the periphery of the ellipse at the point of cutting, which is the condition sought for.

The mechanism may be adapted to the turning of ellipses of different eccentricity by simply adjusting the distance of the crank-pin from the center of the shaft, D, so that its value will be

$$R = \frac{a^2 - b^2}{a^2 + b^2} c.$$

It is evident that if a pair of rotary cutters be substituted for the turning tool, in trimming sheet metal blanks, the required result will be obtained in a similar manner.—*Franklin Journal.*

THE LIGHTHOUSE OF AR-MEN, FINISTERRE.

In the year 1881 will be completed a work which, for boldness of conception and perils involved in its execution, ranks among the most notable ones of the kind in modern days. We refer to the lighthouse of Ar-men, at Cape Finisterre, Spain, which, begun in 1867, will be lighted up during the course of the coming summer. As certain details of its structure have not hitherto been well known, we present herewith an engraving of the lighthouse, along with a brief description of it. The Ar-men rock is one of the numerous heads of the submarine prolongation called the "Chaussée de Sein de la Pointe du Raz," which enters Cape Finisterre between the Bay of Douarnenez and that of Audierne, and of which the Isle of Sein, at about seven miles from the mainland, is one of the first uprisings. The sinister name of "Bay of the Dead" given to the indentation at the extremity of this point of the continent, says enough to show

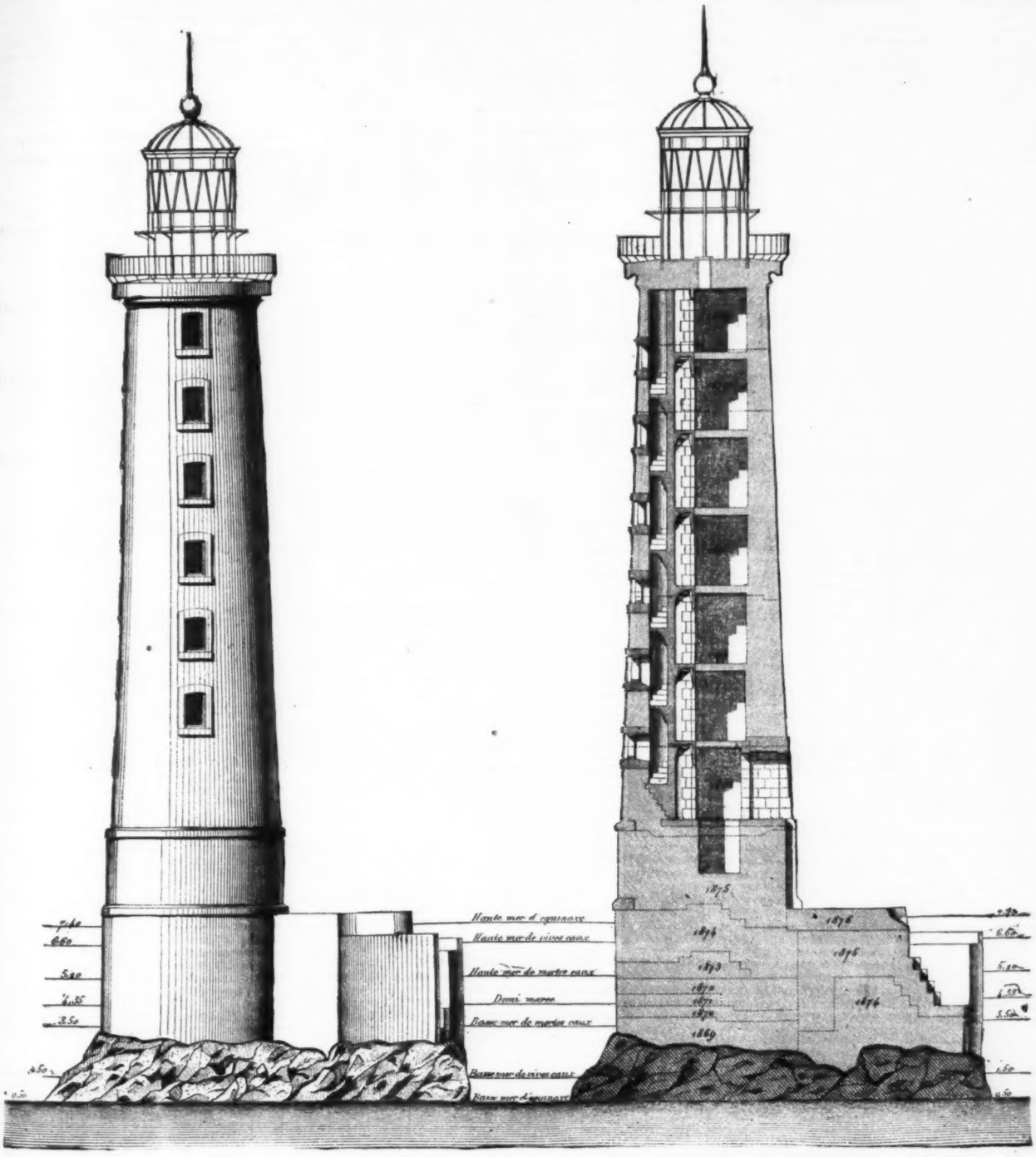


FIG. 1.—ELEVATION.

FIG. 2.—VERTICAL SECTION.

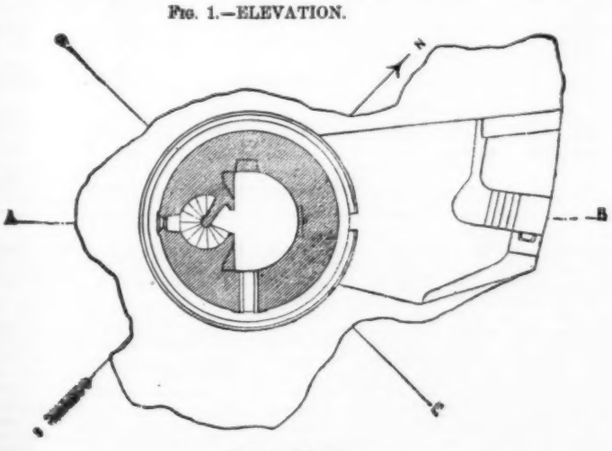


FIG. 3.—PLAN.

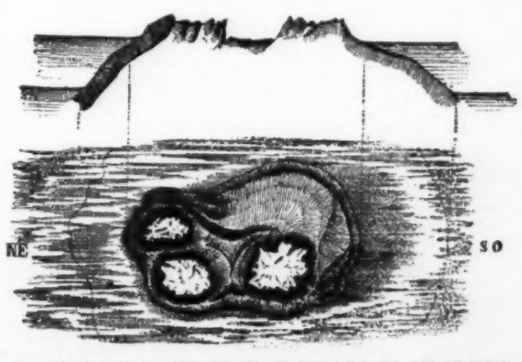


FIG. 4.—ELEVATION AND PLAN OF THE ROCKS OF AR-MEN.

THE LIGHTHOUSE OF AR-MEN, FINISTERRE.

the terrible reputation that this locality possesses. The Armen rock, the only choice that offered itself for the construction of the lighthouse, is only 24 or 25 feet wide by about 50 feet in length, and is formed of hard gneiss emerging from the sea at three points, which at low tide are only uncovered for a height of about five feet. Against these rocks the waves dash with such constant fury that it was found impossible to set foot on them till after a whole year of vain efforts. And on such a spot as this has at length been erected a tower in masonry ninety feet in height, and which now only awaits the necessary light to ward off from the spot all but its intrepid guardians!

It was not till after two years of labor that the engineers finally succeeded in fastening in the rocky foundation, to a depth of about a foot, the fifty-five iron bars, 2 inches in diameter, and spaced 3·25 feet apart, which were to consolidate and anchor the future masonry; and in this two years the rocks could only be approached twenty-three times; only twenty-six hours' work could be performed, and the expense of the whole amounted to $5,800. As the general mode of construction has been before described, we may pass on to notice the general arrangement of the structure. When a lighthouse is built on land the principal thing to be thought of is making access to the lantern as easy as possible. The custodians live in neighboring houses, which may be extended at leisure, and as occasion may require, the expense being the chief thing to look after. But in lighthouses at sea, and especially in the one under consideration, the conditions are entirely different. Two custodians out of three, or three out of four, must live in the tower. The houses on land are only used as a place of rest after the custodian has been for some time in active service. At Armen, everything has been arranged in such a way as to provide for an enforced isolation of six months, and the way this has been accomplished will now be described. The lighthouse may be divided into four distinct parts: (1) The foundation; (2) the base; (3) the hollow tower; (4) the lantern and the illuminating apparatus. The foundation is cylindrical, 18 feet high, and 24 feet in diameter, the latter being the maximum dimensions that the rock permitted. It is solid, and is prolonged to the northeast by a landing platform provided with a stairway.

The cylindrical base is of the same diameter, and has in its center a hollow designed for storing coal, of which it will hold about five tons. Above this arises the tower, whose diameter at the base is 21·6 feet, and at the top 16·4 feet. It is divided into seven stories (including ground floor) by iron flooring, and which communicate with each other by means of a winding stairway set partly in the wall. The internal diameter is about 10 feet at the first story, 7·4 feet at the second, and 11 feet at each of the others, giving for the extreme thickness of the walls 5·4 and 2·6 feet. A vertical ladder furnishes access from the landing to the first floor, which gets no light except that which enters through the door. On each side of the passage are the water tanks. The remaining stories are lighted by windows looking toward the southwest. In the second story are kept the oil tanks, which are of sufficient capacity to hold a supply sufficient for six months' consumption. The next story above is devoted to the purposes of a kitchen. The two above this serve for sleeping apartments for the keepers. The succeeding story is reserved for the steam fog horn.

Finally, in the top story is the chamber in which are deposited the lamps, wicks, chimneys, etc.

As many closets, shelves, etc., are provided as the limited space will permit, for the storage of provisions for the custodians; and calculations based on storage room in sea-going vessels for a like purpose, show that sufficient space is disposable here for enough food to last six months.

The fog horn, to be used in foggy weather, is provided with a small Cagniard-Latour siren, actuated by steam. The small engine for the latter purpose is placed immediately underneath. To provide a sufficient quantity of fresh water for the keepers a distilling apparatus is installed in the kitchen chimney. The tower is crowned by a cornice surrounded by a galvanized iron railing, and above it is the lantern. The light will be a fixed white one of the second class, that is to say, it will consist of a lamp with five concentric wicks whose diameters are respectively 105, 85, 65, 45, and 25 millimeters. The intensity of the light will be equivalent to eight hundred and seventy-eight Carcel burners, and the maximum range will be about forty miles.

THE FAILURE OF A DOCK WALL.*

By LIEUT. H. D. LAFFAN, R.E.

ABOUT the beginning of July 1879, a serious settlement took place in the south wall of the East India Import Dock. The wall in which the failure occurred was built of brick and stone lime mortar. The brickwork was apparently very good, the wall having in one place sheared straight through the bricks, leaving the joints undisturbed.

For a length of about 700 feet the wall had sunk down to a depth of several feet, and in its final position had assumed an irregular wavy outline, the greatest bulge (at the footings) being 7 feet 3 inches, and the greatest subsidence 5 feet 3 inches. The forward movement, however, was almost entirely confined to one point, and was probably caused by a heavy crane fixed just behind the top of the wall on an iron framework. As the wall sank the crane fell backward, acting with a leverage upon the wall and driving it forward.

At the western end of the settlement the wall had sheared downward, leaving a fissure 1 foot wide, with a step 2 feet high at the top; at the east end the shearing was distributed over a number of smaller fissures. No part of the stuff at the back of the wall had found its way into the dock, but had sunk down, forming a trench behind the wall, in which, in some places, there was a depth of as much as 15 feet of water.

A large warehouse, only 50 feet distant from the edge of the wall, had shown no sign of settlement; this may be accounted for by the fact that they were built on a solid layer of concrete, 13 feet thick. The dock wall in which the failure occurred was built without a foundation of piles, upon cross planking, laid on gravel and sand some 2 feet deep, overlying running sand, and the settlement appears to have resulted from the sand being drawn away from under the wall.

The immediate cause of the settlement appears to have been the tapping of the running sand in the course of excavating works in connection with the Midland Railway Docks about a quarter of a mile distant, when a "blow" ensued which covered in the works with sand to a considerable depth. The sand was allowed to run for several days, and while it was running the settlement occurred in the wall of the East India Dock. At about 5 A.M. one morning the wall sank down suddenly without any previous warning, the greatest subsidence at first being 3 feet, but it afterwards continued to sink gradually as long as the flow of sand in the Midland Railway works continued, and when this was stopped the settlement ceased.

At one time there was a channel from the bend of the River Lea, into the River Thames, about where the Midland Railway Docks now are, and although the channel has been stopped, there is probably still a creek of sand running in this direction through the East India Import Dock, and under its south wall, which has been tapped by the Midland Railway Company. The sand had thus, in all probability, been drawn away from under the wall, which, after supporting itself across the gap for a time, after the manner of a beam, eventually gave way suddenly under its own weight.

This accident has occurred at a time when there is a pressing want of accommodation on the quays of the East India Docks; it has, therefore, been thought desirable to widen the wharf on this quay, as well as to restore it as rapidly as possible. For this purpose a wooden staging is to be erected about 30 feet to the front of the present wall; the wall itself will be left as it is, and a mass of concrete, resting against its base and sloping up to the top, will be laid between the wooden staging and the wall, in order to prevent any further forward movement.

* From the Papers of the Royal Engineer Institute.

APPROXIMATE RULES FOR THE PENETRATION OF ARMOR.

THE examination of the results of firing at armor plates recently has caused a very simple rule-of-thumb to be perceived, which differs from ordinary rules of this kind in the fact that it admits of mathematical proof if two assumptions are made. The formula which we have before spoken of as the official one employed in the department of the Director of Artillery give very accurate results as long as comparatively thin plates were employed. It became apparent, however, when trials were made on a greatly increased scale that the results obtained by this formula were much too large. By a curious coincidence this was not practically felt, because the thicker targets were constructed on the plate-upon-plate system, that is to say, the armor did not consist of one solid plate with backing behind it, but of three or four plates with layers of wood sandwiched between them. This class of armor can be penetrated with less force than solid plates consisting of the same thickness of metal—in the proportion of about nine to ten. Thus it happened that results which were calculated on a system intended to apply to solid plate, answered fairly well for plate-upon-plate targets. When, however, an experiment was made against solid plates of great thickness, the error became apparent. The rule-of thumb we have spoken of gives results which are too large for thin plates, but are sufficiently correct to furnish a rough estimate, and it is so simple that it can be applied almost instantly to any case. It is as follows: Each thousand feet striking velocity of a shot represents the penetration of a plate 1 caliber thick. That is to say, a 9 inch shot, with 1,000 foot-striking velocity, may penetrate about 9 inches; one with 1,500 foot-striking velocity, 13½ inches, and so on. Penetration must here be taken to mean that some portion of the projectile gets through the entire plate. The results obtained, while by no means accurate, are too closely approximate to be the results of chance. The proof is as follows. Two assumptions are made: (1) that the resistance of an armor-plate increases with the square of its thickness; (2) that the weights of all projectiles are proportioned to their caliber (that is $W = cD^3$). The official formula on the first of these two assumptions becomes, when W = weight of shot, v = striking velocity, 2 R = D or caliber, g = the force of gravity, penetration or

$$b = \sqrt{\frac{W v^2}{2g \times 2\pi R \times c}}$$

Collecting all the constants under the letter K we have:

$$b = \sqrt{K \frac{D^3 v^2}{D}}$$

$$b = \sqrt{K D^2 v^2}$$

$$b = D v \sqrt{K}$$

Experience shows that the constant part of this expression $\sqrt{K}$ is about $\frac{1}{1000}$. In other words, that the penetration in inches is equal to the caliber multiplied by the number of thousand foot-striking velocity. The degree of approximation that is obtained by this rule may be seen by the following examples:

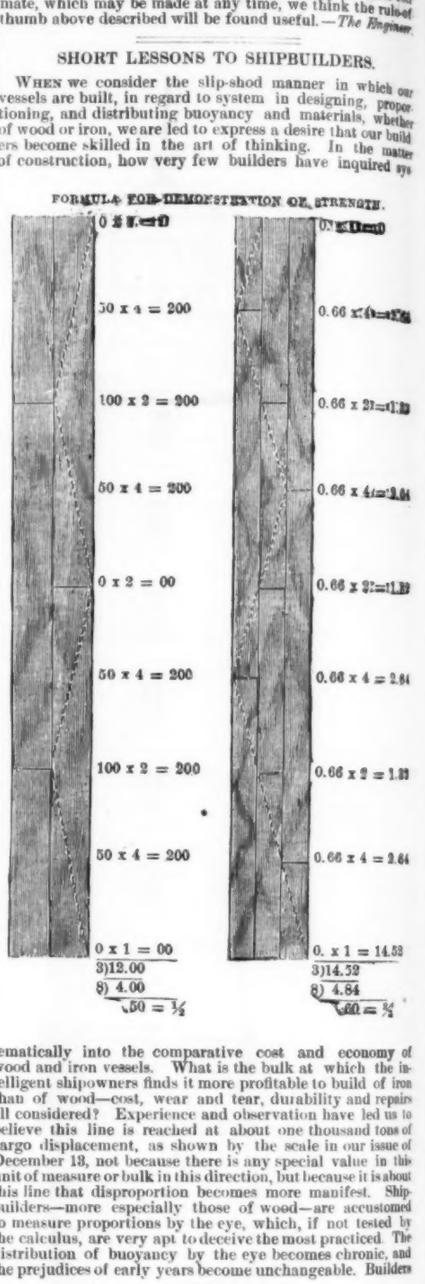
Armstrong new type 8in. gun.

No. of round of series.	Weight, lb.	Velocity in feet-seconds.	Thickness of plate in inches.	Results actually obtained, distance of point from face of plate.	Calculated results — By official formula.	Calculated results — By rule of thumb.
3	182·3	922	6	12·3in.	5·88	7·38
6	—	1139	8	Practically T.	7·53	9·39
7	—	1296	10	9·4in.	9·01	10·37
10	—	1648	12	Practically T.	12·15	13·13
11	—	1800	(12 + 2)	14in.	14·14	14·88
14	—	2101	16·5	20·95	16·28	16·81
54	—	2212	16·5	16·75	17·56	17·7
			35-ton Guns.			
On June, 1872..	700	1300	12½, plate upon plate.	Head through.	16·2 Corrected. 18·06	15·6 Corrected. 17·9
			38-ton Guns.			
October, 1876 ..	812	1420	10½, plate upon plate.	Through, except base.	19·4 Corrected. 21·56	17·75 Corrected. 19·7
August, 1877 ..	817	1419	16½; solid.	Through.	19·3	17·5
			80-ton Guns.			
May, 1877	1700	1585	22, plate upon plate.	About 28, and bending.	30·25 Corrected. 33·7	25·88 Corrected. 29·18

The correction in the calculated results for those rounds when the plate-upon-plate targets is fired at is made by dividing the estimate for solid armor by nine and multiplying by ten. It will be seen that the rule-of thumb gives results which are too small by about an inch in the small plates, while in the thick ones it furnishes more correct results than the official formula. Colonel Maitland, the superintendent of the gun factories, has worked out a formula which gives really good results, especially in the case of thick plates. He has constructed diagrams on which the results can be measured off on diagonal scales with a considerable degree of accuracy. We regret that it is very difficult to do justice to such diagrams by means of cuts, as the lines become indistinct unless rendered on a large scale. Colonel Maitland in his formula takes the resistance of the plate proportional to the square of its thickness, and he employs a term containing a correction such as would materially assist the rule-of-thumb. In fact, Colonel Maitland's formula has evidently elements contained in it closely according with those on which the rule-of-thumb is based, but is of course very much more accurate. His diagram enables good results to be very quickly obtained. For an approximate estimate, which may be made at any time, we think the rule-of thumb above described will be found useful.—*The Engineer.*

SHORT LESSONS TO SHIPBUILDERS.

WHEN we consider the slip-shod manner in which our vessels are built, in regard to system in designing, proportioning, and distributing buoyancy and materials, whether of wood or iron, we are led to express a desire that our builders become skilled in the art of thinking. In the matter of construction, how very few builders have inquired systematically into the comparative cost and economy of wood and iron vessels. What is the bulk at which the intelligent shipowners finds it more profitable to build of iron than of wood—cost, wear and tear, durability and repairs all considered? Experience and observation have led us to believe this line is reached at about one thousand tons of cargo displacement, as shown by the scale in our issue of December 13, not because there is any special value in this unit of measure or bulk in this direction, but because it is about this line that disproportion becomes more manifest. Shipbuilders—more especially those of wood—are accustomed to measure proportions by the eye, which, if not tested by the calculus, are very apt to deceive the most practiced. The distribution of buoyancy by the eye becomes chronic, and the prejudices of early years become unchangeable. Builders are likely to retain these prejudices during life, whereas, had they allowed the sun of science to look in upon their labors, they would have discovered that it requires all minds to know all things.



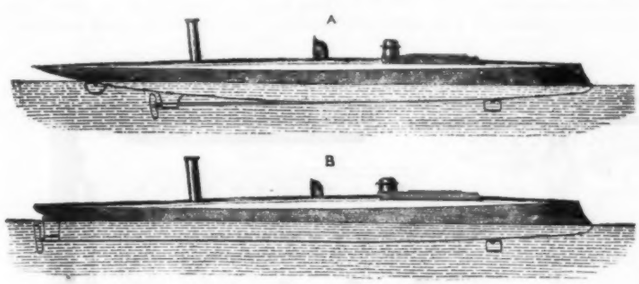
boat frame has to bear the cost of machinery, if we stop there, but if divided between fifty vessels, the cost becomes insignificant, while the advantages become immeasurably great. We shall apply our remarks from time to time to all parts of the vessel, and show that economy of life and property is on the side of better vessels. In regard to iron and steel vessels, we may apply the same remarks with equal propriety. Metallic vessels are out of all proportion; they are weak in all their connections, and show unmistakably that their builders are not men whose judgment is to be relied on in this department of mechanical and constructive science. In any department of literature we should distrust the ability of a man who spelled wagon with two g's. So in metallic vessels. We may well falter in our judgment of the man's ability who, after girdling his vessel in all directions with rivet holes, insists that the vessel is as strong as before, because the holes were plugged with rivets. —*American Ship.*

NOTE.—It will be seen that the three timbered frames in the right hand diagram has sixty-six per cent. of the strength of a single timber, while the double-timbered frame on the left furnishes but half the strength of a single timber—and yet the three-timbered frame occupies no more space in the vessel than the double course. The better distribution of butts makes the difference. It will also be observed that the butts of planking would come on the middle timber, leaving a whole timbey for fastening each plank.

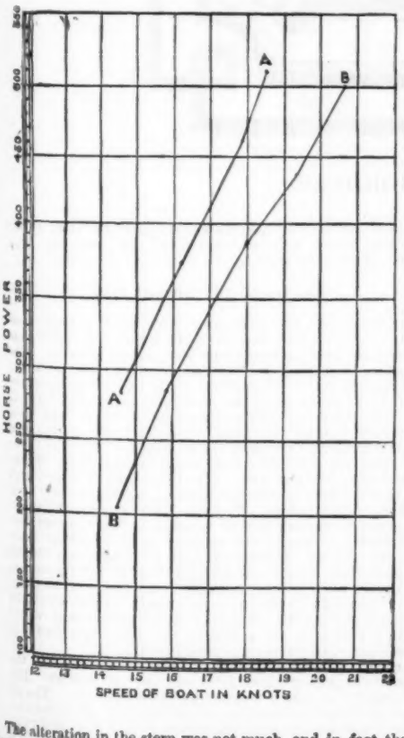
EXPERIMENTS ON THE POSITION OF SCREW PROPELLERS.

IN the early part of last year Messrs. Yarrow & Co. tried some very interesting experiments with one of their first-class torpedo boats, with a view to test the practical value of placing the propeller under the bottom of the boat, as done by Messrs. Herreshoff, and through the kindness of these gentlemen we are enabled to place the results before our readers.

The boat was what is generally termed a first-class torpedo boat, being 86 feet in length by 11 feet beam, and during the trials had a displacement of about 32 tons, and the engines were capable of indicating a trifle over 500 horse power. Boat A illustrates the position of the screw. After repeat-



ed trials, however, it was found that the speed fell far short of what was expected by many who thought that owing to the dense water in which the screw worked a better result would be obtained than when the screw is placed in its usual position at the stern. Such, however, was not the case, as our diagrams show. A series of consecutive experiments at different powers were made, and the curve, A A, on the diagram gives the result. In fact it was found that with the utmost power that could be developed the maximum speed did not exceed 18½ knots—a very bad result for a boat of this displacement and power. It was ultimately determined to place the screw at the stern in its usual position, which was done, as shown in boat B



The alteration in the stern was not much, and in fact the form of the boat, with the exception of what is due to the dead wood and the plating round the stern tube, was the same, the curve of sectional areas being alike in both cases. There was unavoidably a little difference in area of wetted surface, which, however, would operate against the latter form. On trial it was found that the speed obtained with the same machinery and under the same conditions was quite satisfactory; the second curve, B B, on the diagram showing what the results were. This curve represents the best set of consecutive experiments that were made, but the ultimate maximum speed on trial was 21·5 knots. The above results most conclusively point to the importance of studying carefully the best position of propeller relatively to hull. It will be seen that at 18½ knots there was actually a difference of over 100 indicated horse power between the two cases, and this, we think, may reasonably be mainly, if not entirely, attributed to differences in what the late Mr. Froude so clearly proved to exist—viz., the augmented resistance of the hull due to the propeller. We think our readers will agree with us as to the great interest attaching to these and any similar carefully tried experiments.—*The Engineer.*

UNITED STATES OCEAN COMMERCE IN UNITED STATES BUILT SHIPS.*

Mr. President and Gentlemen of the Convention: We are assembled as a national convention for the avowed purpose of the encouragement of the ocean commerce of the United States.

It is the continued depressed condition of our maritime commerce which makes this encouragement necessary, and it becomes the duty of this assembly to suggest or devise measures for its resuscitation.

I beg to premise by saying I have been from early youth until of late years intimately connected with this interest as a builder, owner, and manager of ships; but, having retired from active business, I feel I can look at the matter before us from no interested motive.

I therefore beg to present for your consideration my views as to what should be done to restore our flag on the ocean. Before doing so, however, I wish to allude to the deplorable state of our maritime interests as well as to the causes that have produced this state, and what might result from such a condition of affairs if continued.

Previous to the rebellion, the ship-building and ship-owning interests of our country flourished; the introduction of steam to ocean commerce by our chief competitors (the British) not having then sufficiently advanced to seriously affect the trade which we at that time held and carried on with our sailing vessels.

Unfortunately, our government, *for want of ships*, was unable (during the rebellion) to protect our ocean commerce against the Confederate cruisers; in fact, little attempt was made by the government in this direction, and our ships were consequently in a great measure driven from the sea. Our competitors, taking advantage of the opportunity afforded by our retirement, which they—availing themselves of the rebellion—had forced upon us, rapidly introduced steam vessels into the trade we had previously conducted with sails, and which we were obliged to surrender to them.

When the rebellion was crushed out, and we could with safety go forth to sea again, we found our rivals in complete possession of most of the ocean commerce of the world.

From the experience acquired and the skill attained in building ships in this country previous to the rebellion, we were enabled up to that time to produce both steam and sailing vessels—particularly the latter—of a quality so far superior, and at a price so much less than similar ships could be constructed elsewhere, that all the maritime nations were compelled to seek our markets for a portion of their fleets. Unfortunately, the rebellion increased the cost of construction in this country so much that our former customers were obliged to seek other markets, and Great Britain was patronized to an enormous extent. This fact brought into use (more rapidly than before) iron as a material for ship-building—consequent upon the dearness of wood in Europe suitable and adapted for the construction of vessels. For these reasons, and more particularly on account of the application of steam—not so much in consequence of the use of iron in place of wood, as so often and so absurdly stated—have the Europeans been able to retain the trade which they forced us to abandon by illegally building cruisers for our enemies during our rebellion. Other reasons may be mentioned, such as the comparative high cost of labor in this country, as well as the heavy taxes imposed on ships—as opposed to the low cost of labor in Europe, and low rate of these taxes, especially in Great Britain.

These disadvantages were offset in former times by the superior skill and energy of our people in the building and management of ships, so long as our government extended its fostering care to our maritime interests in the same manner that other governments did to theirs. While our government pursued a course in support of its steam merchant marine similar to that adopted by other nations, we were enabled to produce in this country ocean steamers superior in speed and fully equal in any other respect to those built elsewhere, and our people were making rapid progress toward assuming a position in ocean *steam* navigation equal to that held by them before the introduction of steam.

When, just previous to the rebellion, this fostering care of our government was withdrawn (foreign governments still adhering to their policy of granting pecuniary aid to establish steamship lines with other countries), then did our maritime trade begin to decline, until we have now been comparatively driven from the ocean, and but a very few American-built steamers cross the Atlantic, and rarely is the American flag seen in any port in Europe.

During the rebellion a large amount of American shipping was sold to foreigners; the capital thus released was invested in the building of railroads under a system of enormous land grants—really subsidies which the West was ready to vote for its immediate advantage, but denied to the shipping interests. Land subsidies tax the people; but ocean subsidies bring revenue to the government in the form of increased import duties.

The West and the South, while they refuse to aid the commercial marine, receive continually subsidies from our government through the General Post Office in the payment by our government of the deficit in the receipts for delivery of their mails. Instead of paying subsidies to our own people, we pay them to foreign ship-owners, in the freight money given to their ships, to an amount variously estimated from $90,000,000 to $110,000,000 per annum.

Our chief rival on the ocean has secured by the circumstance of our rebellion and her wise legislation what she failed to achieve by the war of 1812; and while we have listened to her dogma of free trade and the cry of free ships at home, Great Britain, as also the government of the French, has discarded free trade in its ocean commerce, and has continuously fostered that commerce by liberally subsidizing several steamship lines to many of the important ports of the different maritime nations, and granting bounties for building ships. England has outgeneraled us and won peacefully a great triumph in the absorbence of our ocean commerce. This policy has been steadfastly adhered to by Great Britain, as was shown when, a few years since, it was proposed to accept bids from a French company for the contracts for the India, China, and Australia mail service; it was said in Parliament that to receive proposals for carrying Her Majesty's mail from any foreign country would be "free trade run mad," and this objection at once forced the withdrawal of the proposal. Much comment was not long since indulged in by our people as to the possible disadvantage, in case of a foreign war, of our government bonds being held abroad; now, these having to a great extent been returned, we feel relieved. Would it not be much more hazardous, nay, absolutely fraught with national destruction, to give over to foreigners the building of our ships and the production of our navy? Where would we get a navy quickly should we engage in a war with England? Surely she would not build our ships; witness her action toward Russia in their last war, when she prevented delivery of ships built in private yards in England for that government. The French, Prussian, and Italian governments all seek to build their own naval vessels, at whatever cost; and in order that they may have sailors to man them, they encourage their merchant marine, their ocean commerce. We shall never arrive at a settled conclusion of this question unless we approach it on the broad principles of national welfare. We occupy a country having vast extents of seacoasts, with numerous fine harbors, located midway between the greatest trading peoples of the world. Nobody will deny that these coasts and harbors should be protected against attacks of an enemy. This protection can not be adequately afforded without the aid of a navy; we can not maintain a navy without a commercial marine. Even now our government finds very great difficulty in manning the few vessels it keeps afloat. In former times our government found it necessary to grant bounties to fishermen in order to provide sailors for our navy in time of war—nay, even in time of peace. We can not have this commercial marine without ship-builders to construct the ships, seamen to man and sail them, and educated merchants to manage them. We may educate officers at Annapolis, but that institution can not create the sailors necessary for such a navy as this country ought to possess.

The condition of the shipping interest is the gravest domestic question of the present time—one of great national importance. All industries are thriving under the present laws except ocean commerce. But this languishes; and the national weakness and disgrace which result must be a source of alarm and regret to every intelligent and patriotic American citizen.

You will now naturally ask what are the remedies for this unfortunate state of affairs. I propose the following as some which may with propriety and benefit to the whole country be adopted entire or in part by this convention for submission to Congress and the different State Legislatures. Heretofore Congress has been asked to enact laws granting pecuniary assistance to particular lines and to certain corporations and individuals. This course, while it found many friends, was objected to by some on the ground of special legislation. To avoid this and other objections, I would suggest that Congress be requested to pass *a general law* empowering the Postmaster General to establish postal routes on the ocean with American-built steamships of wood or iron or both, in the same manner as he is now authorized to do on land, limiting the cost of such service to a maximum rate per nautical mile. This course would be in entire harmony with the spirit of our institutions and with the uniform policy pursued by the government since its organization. It would, besides, remove all objections on the part of any portion of our people, and secure the vote of every member of Congress in the least disposed to encourage the resuscitation of the merchant marine. Something more, however, is absolutely necessary to be done now that our ship yards and engine shops have mostly ceased to exist, our trade is lost, and our skilled mechanics and sailors are rapidly disappearing.

The import duties on all articles actually used in ship-building, as also those on foreign stores, should be remitted in whole or in part for a fixed and *long period* of time. In addition to these, if a *liberal* bounty should be paid (for a limited time) by our government on each ton of shipping built for foreign commerce, it would materially assist in bringing forth the capital of the people for investment in merchant steam and sailing vessels. Change the law which makes every individual owner responsible for all the debts of a ship, *restricting his liability to his proportion of ownership.* Relieve the ship owner from personal tax on the valuation of the ship; as now, whether tied up idle at the dock or not, making or losing money, the tax approaches three per cent.—a fair interest upon ordinary investments in Europe. Change this to a tax on profits of ship only. Remove all taxes on all materials entering into the construction or arising from the management of ships. Establish a bonded warehouse system which will permit foreign trading vessels to take their stores out of bond. Grant bounties of five dollars per ton of *displacement* for a limited period, say twenty to twenty-five years, on all *vessels built in the United States* and engaged in ocean commerce. Have Congress enact laws to regulate the measurement for tonnage and fix the rating of the ships upon which it will grant bounties, and not depend, as now, for these on the British Lloyds. Modify the laws regulating consular charges, so that instead

* Paper read before the National Convention of ship owning and other commercial bodies for the encouragement of the ocean commerce of the United States, held in Boston, 6th, 7th, and 8th October, 1880, by W. H. Webb, one of the representatives of the Chamber of Commerce of New York

of payment of a sum equal to three months' wages to a seaman discharged in a foreign port, there shall be given the cost of a passage in the steerage from the port of discharge to the port of shipment in his own country, provided that country is the United States, with such additional pay in case of sickness as the consul may determine. Let our government grant contracts to private native shipbuilding establishments for the construction of its ships (as foreign governments do), especially our greatest competitor (Great Britain), maintaining its navy yards for rendezvous, repairs, and equipment of its fleets.

With these aids, I think the building of ships would be promoted, and our people enabled to maintain the higher prices of labor in this country and the dignity of this labor, never known in any other country. Moreover, they would successfully contend, as heretofore, against foreign competition and the pauper labor of Europe; but without such aids we must succumb to our competitors and forego our claim to be considered a maritime people. We shall soon possess neither skilled mechanics to build ships nor men competent to navigate them, and finally no marine. Moreover, we cannot long hope to have a navy; for while we may make officers in our national institutions, crews cannot be created there in sufficient numbers; they can only originate in the merchant service—to which all nations look for the supply of men in time of need.

We have all the materials in this country required and well adapted for shipbuilding in greater quantity and better quality than in any other country. The prices for building sailing ships in this country, as is well known, are less than before the rebellion, and far below those for vessels of equal quality constructed abroad; and, with sufficient encouragement, the prices for steamers, including those built of iron, would soon quite closely approach those constructed abroad. The building of ships in this country keeps down prices and fosters improvements; whereas, if we retire, Great Britain would have a monopoly and our people would soon be obliged, after the second-hand ships of England were disposed of, to pay for their ships admitted free, higher prices than now charged in this country under the present system of protection. It is admitted that our people are well skilled in the construction, sailing, and managing of ships. Then desire the experiment of admitting free ships (as it is called) that all other important interests in the United States (even the building of railroads) are protected by a tariff; why should not the shipping interest be protected, more especially as it was forced to suffer during the rebellion as was no other interest?

PLAIN AND ORNAMENTAL BORDERS.

By J. THEO. CHILD.

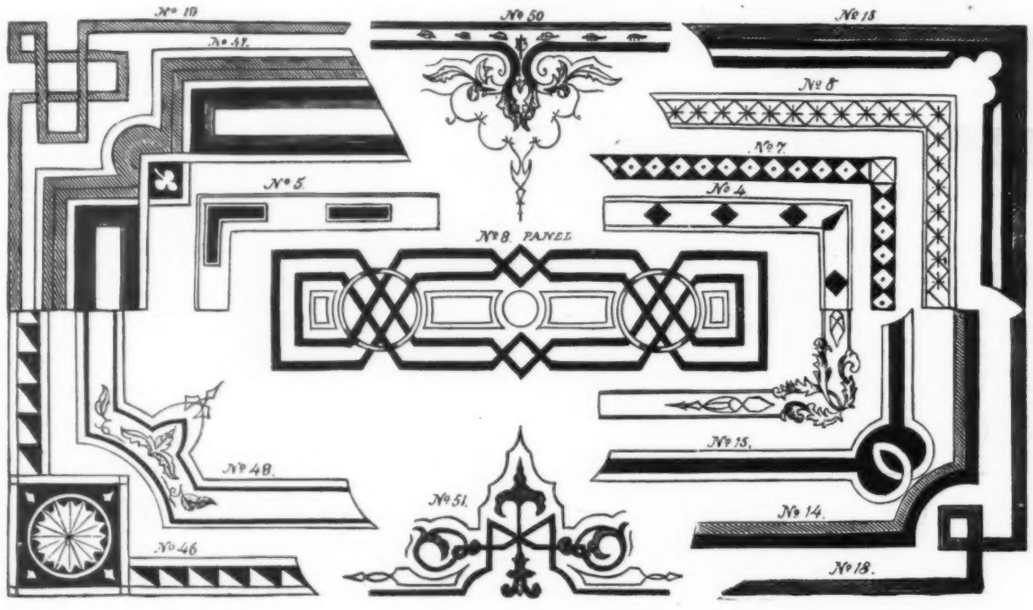
THE accompanying plate contains several designs for plain and ornamental borders, with a corner for each border, and all designed so as to be easy of execution and of practical benefit.

The numbers run irregular from the fact that I selected the patterns from my book of designs, taking those which I considered of most value.

No. 8, besides being used as a border ornament, might be utilized on car head-linings. Nos. 50 and 51 are designed for reliefs on long borders or for center of borders of upright panels. Nos. 46 and 47 are Eastlake patterns, which admit of strong color contrasts, as, for instance, gold on a black ground for No. 46, and for No. 47, red with a gold stripe, a black fine line running through the red; the panel, blue or green, edged and bordered black. The remaining patterns can be treated after similar styles in coloring. In fact, considerable liberty may be exercised in adapting designs to special purposes. Care should be taken, however, to avoid a confused effect, produced where a space is covered with patterns intricately interwoven, or composed of designs that disagree in form. Again, the design as a whole should not appear out of proportion to the space on which it is painted.

This latter defect is noticeable in some car-heads where the design is composed of circular center pieces in connection with broad bands and stripes which would be of proper size for a ceiling fifty feet wide, and at a distance of thirty feet from the eye. The defect named is now, happily, giving place to patterns designed by artists who understand that ornamental designs have utterance, and therefore may be made to express ideas in unmistakable language.

Books may now be had on ornamental work which give certain rules for designing, showing what is the true and what is the false in decorative art. Such works should be studied by the painter.—*Coach Painter.*



A COLLECTION OF PLAIN AND ORNAMENTAL BORDERS.

A LIBRARIAN'S NOTION OF WHAT A LIBRARY BUILDING SHOULD BE.

AT the session of the American Library Association, in Washington, Feb. 10, Mr. W. F. Poole, Librarian of the Chicago Public Library, read a paper on library architecture, which is reported as below in the Chicago *Inter-Ocean*.

Mr. Poole prefaced his paper by repeating what he had said at a previous meeting of the association, in these words: "I know of no better rule to be observed in the library architecture of the future than this: Avoid everything that pertains to the plan and construction of the conventional American library building." For this he had been lectured by the *American Architect* upon the propriety of librarians leaving so abstruse a subject to be treated by those professionally educated in its mysteries. The matter, said Mr. Poole, had been left for three centuries with the architects, and we saw the results.

After eighteen months' consideration of the remarks, which were commented on by his Boston critic, he had found nothing to change or modify. His present purpose was to illustrate their meaning, and to do some construction on his own account. He was convinced that

THE CONVENTIONAL STYLE

of library architecture was very faulty, and there would never be a general reform until better principles were applied to the construction of the largest edifices. The smaller libraries were constantly copying and perpetuating the confessed faults and worst features of the large libraries.

By "conventional American library building" he meant the style of which the Boston Public Library, Boston Athenæum, Astor Library, Cincinnati Public Library, Baltimore Peabody, Congressional Library, and others, were the representative types. All had lofty rooms, and large, open central spaces, surrounded with alcoves and galleries, which were used for the storage of books. Although there were a variety of details in other respects, this was the conventional style of which he spoke.

For illustration, he took the

PEABODY INSTITUTE, OF BALTIMORE,

into which, while it had some objectionable features, some useful appliances and devices had been introduced. The main library hall is 84 feet long, 70 feet wide, and 61 feet high. On the front of this hall is a reading room, 72x30 feet, and in the rear a work room 38½x20 feet, and a librarian's room, 15x20 feet. The alcoves are six stories high. They project 18 feet from the walls, and there is a passage way 2 feet 2 inches wide next to the wall for access between the alcoves, which are 12 feet wide. A sky-light in the roof, and two small windows in each alcove, furnish ample light. The present shelving capacity of the room is 150,000 volumes. The structure considered æsthetically was a beautiful picture; it was the nave and aisle of a Gothic church of the Middle Ages, with the classic associations of five centuries about it, brought down to the practical uses of a modern library.

The objections to it on the grounds of utility and convenience were, in the first place, a wastefulness of space, in the central portion of the building. Books were shelved only in the aisle. The nave was empty, and served only for architectural effect. More than half the capacity of the room was wasted. In some libraries the corresponding floor was used as a reading room, for which it was unfit, as would be shown. The Peabody trustees had the good sense to provide a more suitable room for this purpose. The second objection was the difficulty and expense of heating. While the upper strata of the atmosphere were intensely heated, the lower strata in which we lived had a comfortable temperature. This arrangement was a wasteful expenditure of heats. The third objection was to the shelving of books in the galleries, and, especially in this case, when the alcoves were piled one upon the other six stories high. Under this objection came three others—a wasteful expenditure of the physical strength of attendants in reaching the books, and of the time of readers in waiting; the bindings of books perishing from heat, and the higher up the more destructive was this agency. The red powder which sometimes was found on books stored at great heights was no doubt the ashes of the bindings which had been consumed by excessive heat. Books should, therefore, be

SHELVED IN THE COOLEST PART

of the room, and where the air was likely never to be overheated. That place was near the floor. The fault complained of was often found in private libraries. The objection to shelving books in galleries was that there was no necessity for it. The books could be shelved upon the floor where convenience and preservation required them to be. The folios and quartos should be shelved in wall cases extending around the room, and the royal octavos and smaller volumes in double cases, open on both sides, three feet apart, the side alleys three and a half feet wide, and the central alley five feet wide. The cases would not be so high but that a person of average stature could reach every book without step or ladder. The rule for estimating the shelving capacity was to allow 25 volumes for each square foot of flooring. Applying this estimate to the Peabody Institute, the present shelving capacity of which was 150,000 volumes, there would still be space for 10,000 volumes in the library room proper, utilizing all the rooms in this structure. According to this rule the storage capacity could be increased to 717,000 volumes. This arrangement would allow the division of a library into four departments of knowledge, each having a separate floor.

Another general objection to present libraries was the difficulty in getting about from one part to another. The fifth general objection was the insecurity from fire. The alcoves piled one upon another created draughts. Numerous instances were here given of the destruction of large and valuable libraries. Library buildings should be constructed in a series of fireproof compartments. The principle had been applied in the great ocean steamers. The objection to the use of the great open space referred to for a reading room was that it did not afford the quiet necessary for study.

A seventh objection to the present construction was that it afforded no means for subsequent enlargement. It certainly should

why give up all this trade and commerce, drop out and admit free ships, and settle down into the pauper classes of Europe?

Nearly the entire cost of a ship consists in labor, either in the manufacture of tools (which would have to pay a duty if imported) or in the conversion of raw material; so with the manning and management; and if it is the policy of our people to encourage home manufactures, surely this interest demands protection. Something more than a bounty on the building of the ship is necessary, though this reduces the first cost and lessens the interest account—an important consideration in the contest with the foreign ship-owner, who can obtain money to build ships at half or one-third the rate demanded in this country. Further aid must be extended to the ship-owner; for the great difficulty in resuscitating our ocean commerce is not the first cost of the ships, but the continuous greater expense of maintaining them in service than is demanded from their foreign competitors. Our government should modify the laws imposing taxes or dues upon the shipping interests to an absolute minimum, and, if possible, repeal all such laws. But if this cannot be done without infringing upon existing treaties, or giving to our competitors equal advantages, then let the required notice be given for the abrogation of such treaties, and let others be made embodying the principle of protection for home industry in treaties as well as in a tariff. Let the government continue in a more extended sphere the system inaugurated by our present Secretary of the Treasury, giving statistics to the public and disseminating information in regard to the trade and wants of other countries. Awaken an interest in the subject by enlightening the people until they demand action by Congress; persevere, as our people interested in the manufacture of metals have done, in the matter of the tariff, and once again prosperity and glory to our flag will come. When this matter is brought before Congress and our Legislatures, as it must be, we should be prepared to show the agriculturists how they would be benefited by a revival of our ocean commerce, and the manufacturer that a severe depression in any branch of an important business affects all others.

In conclusion, I would say to those of our number who

NOT BE EXTENDED HEAVENWARD.

Should transepts and a chancel be built so that the plan would represent the true ecclesiastical cross? Whatever might have been the sentiments of piety existing among librarians of the past, it was time to adopt secular common sense in the means to be used in the attainment of practical ends. Any plan of library architecture was faulty which did not foresee and provide for future enlargement. The Boston and Astor libraries were confronted by great difficulties when enlargement became necessary. Division of alcoves with double cases, and the erection of additions had been resorted to at great expense.

The eighth objection was the great cost of present structures in comparison with simpler, less pretentious, and more convenient buildings. The Peabody cost $342,000; Boston, $335,000; Cincinnati, with a capacity of 250,000 volumes, $350,000; and the Astor, two buildings, capacity 300,000 volumes, $398,000. An old proverb was, "It is easy to criticise; it is not easy to construct." Mr. Poole offered his plans to be criticised. His only purpose was to secure better principles in our library architecture. He proposed the construction on a lot 200 feet square of a building with a capacity of 1,000,000 volumes, and the same lot without cramping the space for storage, or changing anything that had been constructed, to provide for 2,000,000. It was not proposed to erect at once more of this building than was needed to meet present wants, but that additional compartments of similar construction should be built from time to time as they were needed. He proposed, first, to secure as nearly as practicable a fire-proof building; second, the reduction of waste room to a minimum; third, spacious and convenient

HOUSE-DRAINAGE, SEWERAGE, AND VENTILATION.

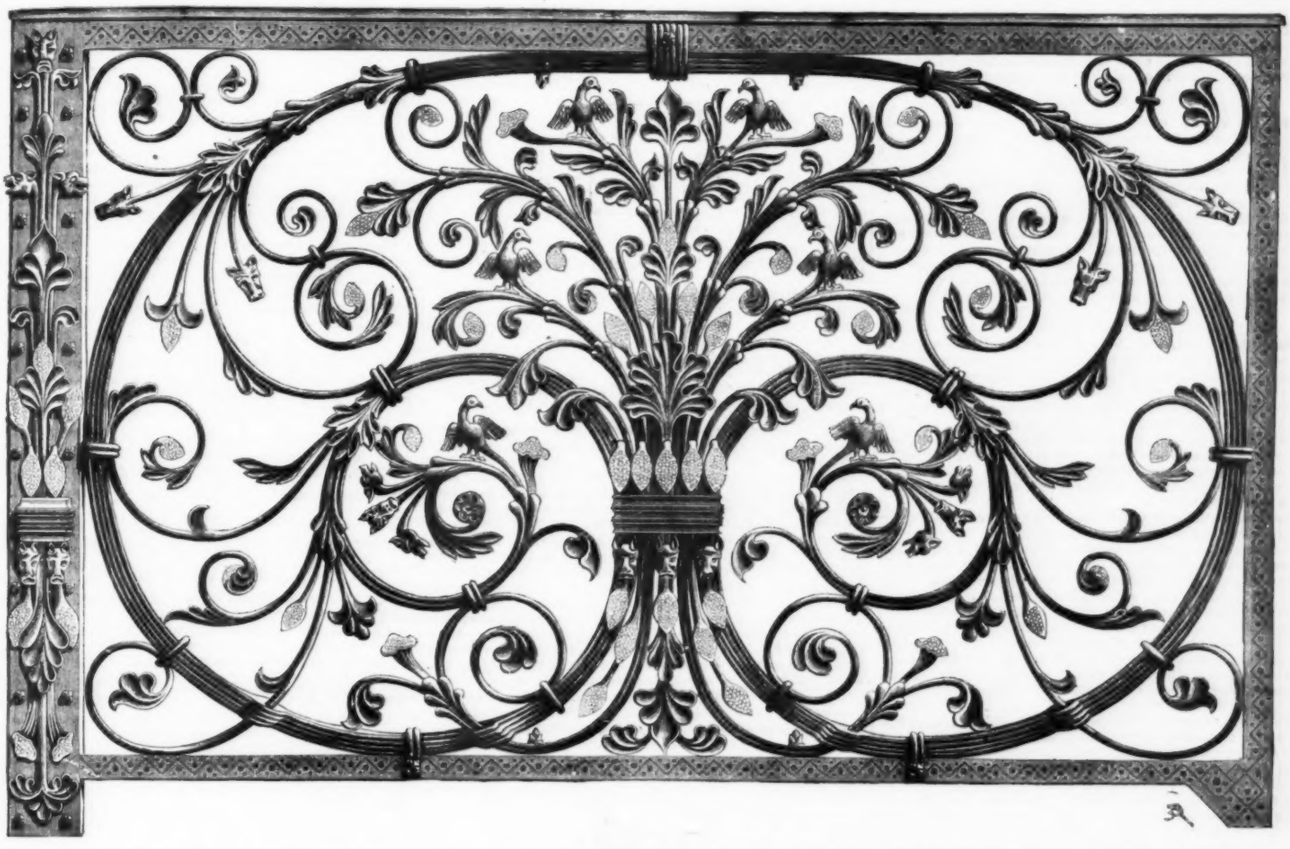
A PAPER upon these subjects was lately read before the Society of Civil and Mechanical Engineers, London, by Mr. Reginald Middleton. The author pointed out that in house-drainage sewage matter should be removed as soon as possible and entirely, so that there may be no fermentation, and that when it has left the house there should be no return of the gases evolved from it. To effect this it is necessary that all pipes should be air and water tight; that the drains should be laid, if possible, outside the houses and in straight lines both as regards direction and gradient; that they should be free from the walls, so that the settlement of the foundations may not affect them, and that they should be easily accessible. No connection should be made with the drain inside the walls of the house, and a disconnecting and ventilating chamber, protected by a siphon trap, should be introduced between the drain and the street-sewer.

The closets should be placed outside the main walls of the house, and should be ventilated; the soil-pipe should be outside the house altogether, and should be carried of its full diameter to a point above the roof and not near either windows or chimneys. The pan closet and ᑐ trap should be done away with, and an apparatus used free from angles and without space for the evolution of gas. The waste-pipes from cisterns and safes of baths and closets should be carried into the open air and no further, so that they may act as detecters.

The discharge-pipes from sinks, baths, and lavatories should be entirely disconnected from the main drain or soil-pipe, and should deliver in the open air on to the gratings of

pulsory inspection by officers appointed by the local authorities will satisfactorily meet the requirements of householders and the public.

When the sewage has passed the siphon trap on the main drain the house should have done with it; but such is not generally the case. If the main drain is disconnected from the sewer by a ventilating manhole, any gas which may return will be carried up the ventilating pipe, but as drains are generally arranged it passes into the house. Small pipes and drains should have a gradient producing a minimum velocity in them of 180 ft. per minute, and large sewers should have a minimum of 150 ft. If these velocities are not maintained deposit will take place, and the sewer will become an elongated cesspool. The quantity of sewage to be discharged is constantly varying, but for practical purposes it may be taken that one-half passes away in eight hours. To meet this variation oval sewers have been designed, in which the scouring action is more concentrated than in the circular sewer. In a sewer in which the transverse diameter is two-thirds of the vertical diameter, the radius of the invert is $\frac{1}{6}$, and the radii of the sides $1\frac{1}{2}$ the transverse diameter, measuring 3 ft. 9 in. by 2 ft. 6 in., with a gradient of 1 in 900, will discharge 1,000 cubic feet per minute with a mean velocity of flow of 167 ft. per minute, and half that quantity with a velocity of 151 ft. per minute. This provides for the discharge of the sewage proper of a town of 200,000 inhabitants. If, however, the rainfall on an area of four square miles at the rate of 1 in. in 24 hours, 80 per cent. of which finds its way into the sewers, be added, the dimensions will have to be increased to 7 ft. 6 in. by 5 ft., and the velocity will be, with the same gradient and running full, 241 ft. per minute, while, when discharg-



SUGGESTIONS IN DECORATIVE ART.—PANEL FROM THE COMMUNION TABLE IN THE CATHEDRAL OF VERDUN, DESIGNED BY A. G. MOREAU, PARIS.—*From the Workshop.*

quarters for the business department; fourth, no overheating of books, or climbing to reach them; fifth,

GREATER FACILITIES FOR COMMUNICATION

between the various departments; sixth, quiet accommodations for students and readers; and, seventh, cost of construction within reasonable limits. On the side most appropriate for the main entrance he placed the central structure 60x75 feet for the business department. The books would be stored in a series of rooms thrown out as wings from the central building, and extending around the lot. These rooms would be 15 feet high, 50 feet wide, and as long as convenient. The width would be determined by the space that could be lighted by side windows and that could be spanned by girders without pillars. Ten of these rooms, carried four stories high, would furnish forty rooms in the entire structure. Each room would contain books on special subjects, or, in the early stages, on several related subjects. There would be no alcoves. As a protection against fire, each room would be cut off from every other by a brick fire-wall, extending through the roof. Every floor would be made thoroughly fire-proof. Access could be gained by elevators and stairways. The capacity of the building, figured on the basis already given, would be 2,000,000 volumes and two stories could be added in case of need for 1,000,000 more. By an extension of the front wings 100 feet on each side, and carrying them back to the rear line, the capacity could be increased 3,000,000 volumes more. Such a building could be constructed on a business block in Chicago. A circulating department could be provided for in one of these rooms. The cost of such a building, as estimated by Mr. W. H. Wilcox, who had built many large structures, was $760,000, exclusive of shelving and furniture. These two items would increase the cost to $870,000.

stoneware siphon gullies, which, again, should discharge into a manhole on the main drain. No trap of any sort in direct connection with the main drain should be placed inside the house. The drain should be ventilated from the siphon rear, the sewer to its extremity.

Many cowls and other mechanical contrivances have been devised for increasing ventilation; but, in the opinion of the author, they either fail when most wanted or are unnecessary. Cisterns should not be made of lead, but preferably of galvanized iron; they should be carefully covered in and easily accessible and frequently cleansed. There should be separate cisterns provided for flushing closets, and it should be impossible to draw any water from these except for that purpose. No cisterns used for dietetic purposes, or for anything but the flushing of the closets, should have any connection whatever with any drain, trap, or soil-pipe.

The main drain must be laid to such a gradient that it will be self-cleansing. When such a drain goes under a house, which cannot always be avoided, the best material to be used is cast iron jointed with tarred spun yarn, and leaded, and laid in a bed of Portland cement concrete. A diameter of four inches is amply sufficient for the drain of an ordinary house. A hope was expressed that before long wrought iron will supersede lead for pressure pipes.

House-inspection seemed to be little understood by householders. A mechanic is well fitted to examine into the condition of the pipes and appliances, but of the designing of sanitary arrangements he knows nothing, or less than nothing, for what he does know is faulty. It is almost useless to call in an engineer to examine a house unless he sees the whole of it, and it is equally useless to ask him to certify as to the condition of the house, if the work has been carried out by the local builder from his report, unless he has seen the whole of the reconstruction. He did not think that com-

ing 500 cubic feet, the velocity will be 151 ft. per minute. Drains must be provided for the removal of rain water, but these may be taken to the nearest stream, and may be short and small, whereas if the sewer is used for this purpose it necessarily goes on increasing in size up to its outfall. Where pumping has to be resorted to, the introduction of rain water into sewers is especially to be avoided. However desirable it may be that the separate system should be adopted, there are difficulties in the way. Two drains have to be provided, and workmen are so careless and ignorant that, unless special precautions are taken to prevent it, they will join up the house drains to either conduit indiscriminately; this arrangement would also add to the complication of our underground works. As the maximum velocity in drains should be about 270 ft. per minute, these cannot be laid down steep hills, but should be contoured round them. In low-lying ground it is difficult to maintain a velocity of 150 ft. per minute, and here in many cases flushing or pumping must be resorted to. If lift pumps are used, the expense of plants, buildings, and ground is serious, besides the constant yearly charge for fuel, repairs, and attendance.

The greatest care must be taken that the foundations of sewers are sound, and that the material employed is well adapted to resist the acids contained in the sewage and the erosive action of water. Portland cement concrete pipes make good sewers for the smaller sizes, and similar concrete laid *in situ* would be better than brick for the larger sewers. House junctions should be laid in when the sewer is built, even if not at once used, and they should point in the direction of the flow. All angles or causes of retardation should be avoided, as also any splashing, which will cause deposit. All sewers should be laid in straight lines, both as regards direction and gradient, so that their position may be

known without difficulty, and they may be easily inspected and cleansed. Sewering through the streets is to be preferred to draining along the backs of houses. A very useful arrangement, where the fall of the drains is small, and where it is desired to irrigate on a small scale, is the flush tank, which, when full, discharges its contents automatically by means of a siphon.

The author considered the arrangement designed by Mr. Isaac Shone, of Wrexham, superior to that of Mr. Rogers Field, both in its certainty of action and in its cheapness. Both these arrangements were good protectors against the return of sewer-gas. The best mode of disposal was to put sewage on the land, and the ordinary sewage farming seemed to be the most natural and simple method. Intermitted filtration is only the same thing in a condensed form. Sewage farms do pay, he stated, when not carried out on too grand a scale, and where not charged with amounts which in no way belong to them. The sludge is troublesome, but it may be collected in tanks, dried and mixed with bone-dust or otherwise, and used as a separate manure; or it may, in some cases, be plowed into ground. For the success of sewage farms the separate system is almost necessary.

In conclusion, the lecturer dealt with the question of ventilation, expressing his belief that cold air may be introduced into a room without draughts supervening if the aggregate of the openings be large enough, while each individual opening is very small, and if the supply of fresh air for the lights and also for the fire be introduced to them direct.

EVERY MAN HIS OWN SENSITIVE PLATE MAKER.

By J. A. FORREST.

GELATINE DRY PLATES.

CLEAN with liquor ammonia and whiting; rinse the whiting off, and place the plates in a vessel of nitric acid and water—say thirty per cent. of nitric acid. Well wash under the tap and coat with substratum (carefully freed from air-bubbles). Pour a little of the substratum on the plate while wet, and allow it to run to waste; pour on a fresh supply and return it to the vessel, pouring it against the side, to prevent air bubbles. Place in a rack to dry, *in daylight*.

SUBSTRATUM.

Take the white of one egg to twenty ounces of water, to which add an ounce of methylated spirit and about twenty drops of carbolic acid. It is better to add the carbolic acid to the spirit and stir, and then add the whole to the albumen; agitate well, allow to stand until next day, and filter clean. This will keep for months.

I find this a "perfect cure" for frilling; the substratum adheres thoroughly to the glass, and the gelatine adheres thoroughly to the substratum. No alum water is necessary while developing, nor any tampering with the emulsion. I have never lost a single plate from frilling since I adopted this method, but dozens before.

THE EMULSION

Place thirty grains of Nelson's No. 1 gelatine and two ounces of water in a small beaker; add fifty grains of pure bromide of ammonium, stir with a glass rod until the bromide is dissolved, and allow it to stand, say, half an hour. At the same time weigh 150 grains of the same gelatine and place it in another beaker with three ounces of water; set aside with the first lot of gelatine. This is simply to allow the gelatine to thoroughly swell before proceeding to heat it.

Dissolve 75 grains of nitrate of silver in one and a quarter ounces of water (I usually keep this in stock in quantity) and place it in a beaker.

Dissolve 10 grains of pure *carbonate* of ammonia in a quarter of an ounce of water in a small beaker.

Place the beaker containing the gelatine and bromide and the one containing the silver nitrate in hot water contained in any convenient vessel (I use an ordinary lading can). Have ready a common eight-ounce white stone bottle (mine is an old ink bottle); heat this by filling it with boiling water and emptying it out. The gelatine and bromide being thoroughly dissolved by the heat, pour it into the warm bottle; then take the heated nitrate of silver and add it, say, at three times, shaking well each time, to the gelatine and bromide. Light the gas burner under the tin vessel, and allow the water to just boil.

When all the nitrate of silver has been added to the gelatine and bromide, *but not till then*, and the whole has been well shaken up, add the solution of ammonia carbonate and again shake well, *holding the cork tightly in the bottle*. Allow to subside for a few seconds; then carefully draw out the cork to liberate the carbonic acid. Place the cork in the bottle again, and repeat this shaking up two or three times; the water will now be nearly or quite boiling in the "lading can." Place the bottle of emulsion in the latter with a very *slack cork*, and allow to boil for not more than ten or twelve minutes, during which time you have introduced the well-fitting cork and shaken up the emulsion, say, three or four times.

During the time the emulsion is "cooking" the gelatine in the other beaker can be dissolving in another vessel of water, heated to *not more* than 100° or 120° Fah. Under 100° is better. This is important.

As soon as the emulsion has boiled the ten minutes place the bottle in cold water, and rapidly cool down to, say, 90° or 100°. Your hand will guide you by grasping the bottle, but cool down well. Now add it, say, three times the melted gelatine, well shake up at each addition, and the emulsion is finished. Pour it into a dish to set in the coolest place you can find (I use a soup plate, and cover it up with a small tray and cloth to keep it safe from light). It will take an hour or two to *set thoroughly*.

TO WASH THE EMULSION.

Procure a piece of *fine* canvas, such as ladies use for wool work—say fifteen inches square; wet this and squeeze it out, spread it upon your table, and scrape the set emulsion on to it; fold it up by the corners, and screw it tight until all the emulsion has been squeezed through into a basin of water. Then pour water and emulsion into a common glass jug holding about a quart; fill up the jug and stir the emulsion well with a stout glass rod. Allow it to subside, which it will do rapidly, during which time spread the same piece of canvas upon the rim of an old hair sieve, and tie it round with a piece of string, allowing the canvas to "bag" just a little; upon this pour the contents of the glass jug, allowing the water to run into a vessel in which is placed the sieve rim. Then gather the emulsion off the canvas with a silver spoon and put it again into the jug, which is filled with fresh water, and again transfer to the canvas, and thence once more to the jug. Three waters will wash the emulsion thoroughly, and not take more than half an hour. The emulsion is now left upon the canvas to drain for, say, an hour, and I then store it in one of those "bloater-paste" pots which have a ground stopper lid tightening with a triangular fastening.

The foregoing quantity of emulsion, when washed and drained, usually fills two of the pots, each holding about five ounces or a little more; this varies with the gelatine, the latter holding more or less water. To *each pot* I add three drachms of *absolute alcohol*; this makes the emulsion keep for a week at least, but I like to use it up in three or four days.

Do not use distilled water or rain water for any part of the process; tap water, or, better still, spring water, will be all that is wanted. The water is better for being a little hard.

COATING AND DRYING PLATES.

The Leveling Board.—Mine is 3 feet by 2 feet 6 inches, 1¼ inches thick, with two stout battens crossways of the wood underneath. This wants to be pretty stout and rigid, and made of good dry timber. Across the surface there are fastened strips of wood half an inch square and at three inch intervals, as in Fig. 1. These are simply guides in the process of leveling: a lid, made as light as possible, but light-tight, and of sufficient dimensions to cover the entire board, with a flange sufficiently deep (say 3 inches) to cover the board plates, and for the lower edge to rest upon the table.

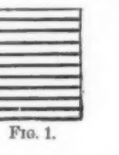


FIG. 1.

To Level the Plates.—I level each plate separately, thus: One of my youngsters makes a lot of little common clay cylinders, by rolling the clay into lengths of a few inches and about half an inch diameter, and cutting it up into about three-quarter inch lengths. Three of these pieces of clay are used to each plate, as in Fig. 2. The pieces of clay are placed on the leveler's board at such a distance as to suit the size of plate, and on each piece of clay I press a gun wad; upon these the glass rests. A circular spirit level, or two ordinary spirit levels placed at right angles upon the plate, enables you to see which way the plate requires pressing down upon the clay supports to set it perfectly level. I have tried all sorts of means of leveling the plates, but find this the best and most simple; a little practice will enable you to level the plates rapidly and perfectly. I never exceed half an hour in leveling sixteen plates.

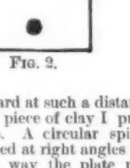


FIG. 2.

Coating the Plates.—During the time I am leveling the plates I have one of the pots of emulsion placed in some hot water in the "lading can" over my gas burner, with just sufficient gas to keep the water hot, *but not to boil*—say at a temperature of 130° or a little more; by the time I have leveled my plates the emulsion will be melted and heated up to the same temperature as the surrounding water. Now commences the dark lantern business. My lantern is a wooden box, 12 inches every way, the front consisting of two panes of glass, which slide in grooves, and are sufficiently long to be easily taken hold of and lifted out. The one glass is ordinary sheet, coated on one side with negative varnish, containing a sufficient quantity of aurine to make it a tolerably deep yellow; the other glass is ordinary ruby. The slides are arranged to leave one inch space between the two glasses; a paraffine lamp is placed inside the box, which is provided with a good-sized chimney, such as are used for magic lanterns. The yellow glass is nearest the light, and the ruby outside; this gives plenty of light, is perfectly safe, and about as cheap and convenient as anything you can get.

FIG. 3.

Proceed to filter the emulsion through a good tuft of cotton wool placed in a funnel. Place a glass marble upon the cotton wool in the center; it prevents the wool rising when the emulsion is poured upon it. Before pouring the emulsion on pass some hot water through the cotton wool, and pass the emulsion through *twice* at least.

The Pouring Bottle.—Mine consists of a blue pyro bottle, close to the bottom of which I have bored a hole, and into this hole I introduce a glass tube, with a portion of India-rubber tube upon it to make it water-tight (Fig. 4), and tie a string round the tube and neck of the bottle to hold the tube in its place. This is my pouring bottle. You will see my object is to pour from the *bottom, thus avoiding air bubbles*. The tube is simply a "feeding-bottle" tube, and the whole thing does not cost a shilling, but is worth a good deal for this job. Heat the bottle in warm water, filter the emulsion directly into it, and set it in a basin of hot water for immediate use.

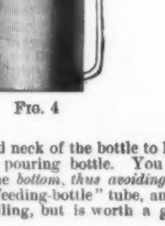


FIG. 4

Coating.—Pour the emulsion out of the bottle into a glass measure (I have several sizes), using a drachm to every 10 square inches of surface; thus, a 10 by 8 will take 8 drachms or one ounce.

I consider it very important to measure the quantity on each plate. Spread with a light bent glass rod, and when all the plates are coated put the lid over, and you can admit white light until they are set, which will probably be two or three hours. Don't disturb them till thoroughly set.

DRYING THE PLATES.

The Drying Box.—This is constructed after a style familiar to many. When the emulsion is thoroughly set on the plates transfer them to the drying box, light the Bunsen burner, and in about twenty hours the plates will be dry.

Storing the Plates when Dry.—Place them face to face, *without anything between them*; wipe the backs from any emulsion that may be upon them, and so place them in pairs. I usually make them up in parcels of eight plates, the box, etc., being adapted for making sixteen plates at one heat. I wrap up in yellow paper two thicknesses and brown paper ditto.

Exposure.—Find out by practice, and according to light and lens.

DEVELOPMENT.

Alkaline bromide of ammonium to be kept in stock in a well stoppered bottle.

No. 1.

Ammonium bromide	160 grains.
Liquor ammonia	4 ounces.
Water	4 "

No. 2.

Pyrogallic acid (best)	48 grains.
Absolute alcohol	2 ounces.

Instead of using the "dropping bottle" for the alkaline bromide, make up a developing solution consisting of two drachms of No. 1 with fifteen ounces of water; then, according to the size of plate you want to cover, use one drachm of the pyro in spirit to each ounce of developing solution. Thus for a half plate I measure into an ounce measure (graduated) one drachm of No. 1 and fill up the measure with the developing solution, and use *at once*. I think this as convenient as any other way; if you prefer to use the pyro dry it is simply three grains to the ounce of No. 1.

These plates develop rapidly and very much like a wet plate; push the development pretty well. The plates will not fog. Fix thoroughly in saturated solution of hypo, and your negative should be equal to any wet plate you ever saw. Wash thoroughly and flow with methylated spirit; this helps the drying.

INTENSIFYING.

This may be necessary, and, under any circumstances, improves the negative when used with judgment (I got the idea from Monckhoven, a first-rate chemist; the intensifier is *perfect*, and based upon *sound chemical theory*).

No. 1.

Two per cent. solution of bichlor. of mercury.
Two per cent. solution of bromide of potass.

Mix together and label No. 1.

No. 2.

Two per cent. solution of *pure crystallized* cyanide of potassium.
Two per cent. solution of nitrate of silver.

Mix by adding the silver solution gradually and well shaking until all the cyanide of silver redissolves; then add, very gradually, a *weak* solution of nitrate of silver until just a little silver cyanide remains undissolved. Allow this to settle thoroughly and decant, and label No. 2.

To Apply the Intensifier.—This can be done immediately after fixing and *well washing*, or equally well *after* the negative has been allowed to dry; but in the latter case it is better to allow the negative to dry without using the spirit. By this means a print can be taken from the negative *before intensifying*, and if of sufficient printing power the intensifier will be unnecessary. When the negative has been allowed to dry, wet it thoroughly before applying the intensifier. Pour at a sweep a sufficient quantity of No. 1 upon the negative, keep it flowing, and the image will gradually become white; wash off under the tap, and allow the negative to soak for a moment or so until the mercurial solution is well washed off. *Don't return the mercurial solution to the stock; it is cheap, and will spoil the rest.* Now flood with No. 2. All the whites of the negative will assume a deep brown purple, and the shadows become beautifully clear; wash well under the tap, treat with spirit, and put in the rack to dry. No wet negative can possibly beat, if equal, the one you should now have; finally varnish.

In conclusion, allow me to say that, in the method I have endeavored to lay before you, there is really nothing or little actual originality, but rather a balance of conditions generally necessary in order to arrive at a satisfactory result. Practice and *careful manipulation* are really important, but the matter is much more simple than all this writing may make it appear.

The method that I adopt is cleared of a lot of complications, and, saving the time the emulsion takes to set before washing, I can get through the making and washing certainly under an hour; and, also, saving the time the emulsion takes to set on the plates, I can level, coat, and get my sixteen plates in the drying box easily in another hour, and put all my traps away; so by making my emulsion one evening (two pots), and coating sixteen half plates in two other evenings, I get thirty-two plates comfortably in three hours, and at a cost of certainly not more than eighteenpence for emulsion and the luxury of a reliable plate, easy of working, sure in its result, and that will not play you any tricks by either frilling or expanding.

SUPPLEMENT.

Use of Carbonate of Ammonia in the Emulsion.—Monckhoven recommended *liquor* ammonia. My experience has been that ammonia added in this form acted *immediately* upon that portion of the bromide of silver with which it came in contact, which, although quickly mixing with the rest, had injured a portion, and more or less fog or yellowness in the shadows resulted. At all events my experience has been never to obtain clean, transparent shadows when using *liquor*

ammonia in any quantity, however small. It then struck me that if the, if any, acidity of the gelatine could be corrected, and the emulsion rendered alkaline gradually, the same end might be obtained without doing mischief; and I determined to try the carbonate, my theory being that the carbonate of ammonia when added to the emulsion only *warm* (say 90° or 100°), would only mix with the emulsion as carbonate of ammonia, the whole of the silver being converted into bromide of silver *before* the ammonia carbonate is added. But upon the application of heat up to boiling point, the carbonic acid is gradually liberated, and the ammonia set free to act and render the emulsion alkaline. I don't know whether the theory is correct, but I find the effect most important.

Adding the Silver Nitrate to Gelatine containing the Bromide of Ammonia.—A great deal has been written regarding the vast importance of adding the silver nitrate drop by drop, and various devices have been introduced for allowing the silver to be so added. Now, this means a great deal of time and trouble, and I never could see why this should be either necessary or advantageous; but experiment alone could satisfy me, so I made repeated experiments, carefully and time-taking, and I found no difference in the result of the two methods—one being to add the silver nitrate solution drop by drop, agitating the gelatine all the time, and the other being to pour the whole of the silver nitrate solution *at once* into the gelatine, and well shake it up. Thus much time and trouble are saved. As a sort of compromise I generally add the silver solution at two or three times, and shake well between each addition, which does not take three minutes; but there is not the slightest advantage over adding it all at once.

Washing the Emulsion.—This, too, has been written up as a formidable undertaking. I never could see why, as the quantity of very soluble salts could be but small if the equivalent proportions of the bromide of ammonia and nitrate of silver were anything near, in the actual quantities of each made use of, and the proportion of two parts of bromide of ammonia to three parts of nitrate of silver is, although not quite correct in theory, quite near enough in practice, and leaves little to wash away. Five hours' washing at least, and innumerable changes of water, have been recommended as absolutely imperative; this, too, I have tested, and I now wash my emulsion in certainly not more than three changes of water in less than half an hour, thus again saving time and trouble.

Coating the Plates.—I consider it important to level each plate separately and to put a given quantity of emulsion on each plate, and that quantity not less than three drachms per ten square inches of plate—a quantity very difficult to retain on the plate unless leveled and left undisturbed until the emulsion is thoroughly set.

PLATE DRYING BOX.

Two galvanized iron trays, containing chloride of calcium, are placed in the two bottom compartments; the trays are one and a quarter inches deep and fourteen and a half inches long, so as not to cover the draught holes.

In putting my plates into the box, I first lay them upon plates of common 15 ounce sheet, 15x12, and put together upon the shelves in the box. You will notice that the air travels over each shelf after passing over the calcium trays. The whole thing is cheap, effective, and simple enough.—*Advance Proof Sheet, Anthony's Photographic Bulletin.*

ACTION OF AN INTERMITTENT BEAM OF RADIANT HEAT UPON GASEOUS MATTER.

By JOHN TYNDALL, F.R.S.*

THE Royal Society has already done me the honor of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question, the latest of whom,† while justly recognizing the extreme difficulty of the subject, and while verifying, so far as their experiments reach, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapors.

None of these investigators appear to me to have realized the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognize the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed, some time ago, the threads of the inquiry. The results shall in due time be communicated to the Royal Society; but, meanwhile, I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvanometer, it occurred to me several weeks ago that the results thus obtained might be checked by a more direct and simple form of experiment. Placing the gases and vapors in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapors ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea. But, at this point, and before my proposed gas thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

From the first I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapors really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds; while highly athermanous bodies would produce loud sounds—the strength of the sound being, in a sense, a measure of the absorption. The first experiment, made with a view of testing this idea, was executed in the presence of Mr. Graham Bell,* and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapors employed in my former researches. My first source of rays was a Siemens lamp connected with a dynamo machine, worked by a gas engine. A glass lens was used to concentrate the rays, and afterward two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about seven inches distant from the lens. A circle of sheet zinc, provided first with radial slits and afterward with teeth and interspaces cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits produced the desired intermittence,‡ while a flask containing the gas or vapor to be examined received the shocks of the beams immediately behind the rotating disk. From the flask a tube of India-rubber, ending in a tapering one of ivory or boxwood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. Graham Bell, the arrangement here described is rude; it is, however, effective.

With this arrangement the number of sounding gases and vapors was rapidly increased. But I was soon made aware that the glass lenses withdrew from the beam its effectual rays. The silvered mirrors employed in my previous researches were, therefore, invoked; and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results which I have now the honor to submit to the society were obtained.

Sulphuric ether, formic ether, and acetic ether being placed in bulbous flasks,‡ their vapors were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disk, so that the intermittent beam passed through the vapor, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapors which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapors. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapors of amylene, iodide of ethyl, iodide of methyl, and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

It is the vapor, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapor-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock-salt cell filled entirely with a volatile liquid, and subjected to the intermittent beam, produced no sound. This cell was circular and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disk and allowing its slits to act like the perforations of a siren.

Thus, as regards vapors, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam, it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas it gave birth to a musical note which, when the beam was in good condition, and the bulb well chosen, seemed as loud as that of an ordinary organ pipe.§ We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand in hand.

A soap bubble blown with nitrous oxide, or olefiant gas, and exposed to the intermittent beam, produced no sound, no matter how its size might be varied. The pulses obviously expended themselves upon the flexible envelope, which transferred them to the air outside.

But a film thus impressionable to impulses on its interior surface must prove at least equally sensible to sonorous waves impinging on it from without. Hence, I inferred, the eminent suitability of soap bubbles for sound lenses. Placing a "sensitive flame" some feet distant from a small sounding reed, the pressure was so arranged that the flame burnt tranquilly. A bubble of nitrous oxide (sp. gr. 1·527) was then blown and placed in front of the reed. The flame immediately fell and roared, and continued agitated as long as the lens remained in position. A pendulous motion could be imparted to the bubble, so as to cause it to pass to and fro in front of the reed. The flame responded, by alternately roaring and becoming tranquil, to every swing of the bubble. Nitrous oxide is far better for this experiment than carbonic acid, which speedily ruins its envelope.

The pressure was altered so as to throw the flame, when the reed sounded, into violent agitation. A bubble blown with hydrogen (sp. gr. 0·069) being placed in front of the reed, the flame was immediately stilled. The ear answers instead of the flame.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was, therefore, great. Placing a small quantity of liquid ammonia in one of the flasks and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sounds may be always intensified. The ordinary temperature, however, suffices in all the cases thus far referred to.

In this relation the vapor of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling point. Placed in the intermittent beam, I heard—I vow with delight—a powerful musical sound produced by the aqueous vapor.

Small wreaths of haze, produced by the partial condensation of the vapor in the upper and cooler air of the flask, were, however, visible in this experiment, and it was necessary to prove that this haze was not the cause of the sound. The flask was, therefore, heated by a spirit flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapor, however, the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but, notwithstanding the tenuity of the vapor, the sound extracted from it was not only distinct, but loud.

Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit lamp until all visible humidity had been removed, and afterward urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapors, and it cannot be otherwise than satisfactory to me to find that particular vapor, whose alleged deportment toward radiant heat has been most strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapor, we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal gas and held bottom upward in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal gas. The flask was then placed upright, with its mouth open upon a table, and permitted to remain there for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.

Transferring a small flask or a test tube from a cold place to the intermittent beam, it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporization by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapors absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapor, would detach the effective rays, and thus quench the sounds. The experiment was made and the conclusion verified. A layer of water, formic ether, sulphuric ether, or acetic ether, one-eighth of an inch in thickness, rendered the transmittent beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapors—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that, with those gases which yielded a feeble sound, the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds the displacement was prompt and forcible.

Received January 10, 1881.

FURTHER EXPERIMENTS.

Since the handing in of the foregoing note, on the 3d of January, 1881, the experiments have been pushed forward, augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

All the results described in my first note have been obtained in a very energetic form with a battery of sixty Grove's cells.

On the 4th of January I chose for my source of rays a powerful lime light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector, the stronger vapors rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those in the lime light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapors.

Abandoning the mirror and bringing the candle close to the rotating disk, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the

---

* From a recent communication to the Royal Society.

† MM. Lecher and Pernter, *Philosophical Magazine*, January, 1881. "Sitzb der K. Akad. der Wissensch. in Wien," July, 1880.

* On the 29th November. See "Journal of the Society of Telegraph Engineers," December 8, 1880.

† When the disk rotates the individual slits disappear, forming a hazy zone, through which objects are visible. Throwing by the clean hand, or, better still, by white paper, the beam back upon the disk, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colors of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colors disappear. The flashes also produce a giddiness, sometimes intense enough to cause me to grasp the table to keep myself erect.

‡ I have employed flasks measuring from eight inches to three-quarters of an inch in diameter. The smallest flask, which had a stem with a bore of about one-eighth of an inch in diameter, yielded better effects than the largest. Flasks from two to three inches in diameter yield good results. Ordinary test tubes also answer well.

§ With conjugate mirrors the sounds with olefiant gas are readily obtained at a distance of twenty yards from the lamp. I hope to be able to make a candle flame effective in these experiments.

rotating disk, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disk, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water

As might be expected from the foregoing experiments, an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three, the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapor had led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks and permitted to remain in the laboratory from the 23d of December to the 4th of January. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due, not to a dry air, but to traces of aqueous vapor.

An ordinary bottle, containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent beam, emitted a faint, but distinct, musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some days, was placed in the beam; the dry air above the liquid proved absolutely silent.

On the evening of January the 7th Professor Dewar handed me four flasks treated in the following manner: Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium; while the fourth contained a small quantity of phosphoric anhydride. They were closed with well fitting India-rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles and sometimes destroys the sound.

A bulb, less than a cubic inch in volume, and containing a little water, lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit lamp, the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb* is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapor. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapor.

The action of aqueous vapor being thus revealed, the necessity of thoroughly drying the flasks, when testing other substances, becomes obvious. The following plan has been found effective: Each flask is first heated in the flame of a spirit lamp till every visible trace of internal moisture has disappeared, and it is afterward raised to a temperature of about 400° C. While the flask is still hot, a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapor by sulphuric acid, is urged through the flask until it is cool. Connected with the ear tube, and exposed immediately to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868 I determined the absorptions of a great number of liquids whose vapors I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtedly twelve years ago that the vapor of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed, and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved in 1861 to be absorbers of radiant heat. I limit myself here to the vapors of patchouli and cassia, the former exercising a measured absorption of 30 and the latter an absorption of 109. Placed in dried flasks and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of patchouli.

Many years ago I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh gas was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask, and permitted to displace the air within it. Exposed to the intermittent beam, its sound was similar in power to that of marsh gas.

The specific gravity of marsh gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well dried flask, the brown vapor rapidly diffused itself in the air above the liquid. Placed in the intermittent beam, a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapor. But my former experiments on this vapor were conducted with obscure heat; whereas, in the present instance, I had to deal with the radiation from incandescent lime, whose heat is, in part, luminous. Now the color of the bromine vapor proves it to be an energetic absorber of the luminous rays; and to them, when suddenly converted into thermometric heat in the body of the vapor, I thought the sounds might be due.

Between the flask containing the bromine and the rotating disk I therefore placed an empty glass cell; the sounds continued. I then filled the cell with transparent bisulphide of carbon; the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light while allowing the rays of heat free transmission; the sounds were immediately stilled.

Iodine vaporized by heat in a small flask yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might indeed have been foreseen that the rays transmitted by the iodine as a liquid would also be transmitted by its vapor, and thus fail to be converted into sound.*

To complete the argument: While the flask containing the bromine vapor was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disk. There was no sensible abatement of the sounds with either bromine or iodine vapor.

In these experiments the rays from the lime light was converged to a point a little beyond the rotating disk. In the next experiment they were rendered parallel by the mirror, and afterward rendered convergent by a lens of ice. At the focus of the ice lens the sounds were extracted from both bromine and iodine vapor. Sounds were also produced after the beam had been sent through the alum solution and the ice less conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapors at a distance of one hundred feet from the source of rays.

Several vapors other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapors of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the vapors of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

* In such bulbs even bisulphide of carbon vapor may be so nursed as to produce sounds of considerable strength.

ON SOME PHYSICAL PHENOMENA.

By W. J. MILLAR, C. E.

IN studying our physical surroundings we have certain conditions or manifestations brought prominently before us, and to these we give the names of force, matter, motion, space, and time. Taking these as enumerated, we have:

1. *Force.*—To arrive at an adequate conception of force is beyond our power, but we are all more or less familiar with many of its manifestations; thus, when by muscular action we bring about some change in the condition of the objects around us we are conscious of an exertion on our part which arises from the resistance offered to our operations. The exertion rendered necessary to overcome this resistance gives us an idea of force, and we say that we have to exert force to oppose the resistance offered.

Again, if we watch a stone which has been thrown upward, as it descends again to the earth we see the action of force. To send the stone upward required an exertion on our part, because of the weight or tendency of the body to fall. The stone in its descent was urged towards the earth by the very resistance or weight which we had opposed in giving it ascending motion, and in this falling tendency we see the action of a different form of force than what obtained in the first case, where the exertion was a muscular action of the arm. The resistance overcome was really another form of force, to which we give the name of gravitation. When a bullet is driven from a gun by the firing of a charge of powder, we know that this is due to the sudden formation of gases whose expansive power drives out the movable obstacle or bullet which assists in hemming them in, and we speak of this action as an explosive force.

In short, we may define force to be that which tends to produce change in the condition of matter.

2. *Matter.*—Matter has been defined as that which is obvious to the senses—substance—what we might call the structural characteristics of the universe. The various conditions of matter are apparent as we look around—the rocks and minerals of our globe, and the liquids upon its surface, with the all-encompassing envelope of air, are to us at once illustrations of matter.

Matter in itself has no power to change its condition. Such change can only come about through the action of force. An illustration of this will be found in the case of the falling stone; without the intervention of some agent, whereby force is exerted, the stone would have remained at rest. The quantity of matter in a body is said to be the *mass* of the body.

3. *Motion.*—From what has already been said it will be seen that motion is a condition of matter when acted upon by force. This condition is very constantly brought before us, whether in the movement of the falling stone, the dread rush of the cannon ball, or in the grander movement of the planetary bodies in their orbits around the sun. Besides these greater movements of matter we have what is known as molecular motion such as in the matter of gases, the small particles of which are supposed to be in very rapid motion.

4. *Space.*—It is obvious that in order to have the movements spoken of, the bodies, or portions of matter under consideration, must be separated, there must be a distance between them for this condition to have effect, and this separation we recognize as space. Thus when the projectile has reached the end of its flight, we speak of the space which it has passed over, and as we pass station after station in an express railway train, we feel that the motion is constantly carrying us onward from point to point, and at the end of the journey we speak of the distance traveled or the space passed over. In this idea of space we have only one dimension or length for our measure. A more extended idea of space, however, arises when we consider the motion of the cannon ball in detail, for here, besides the movement in its line of flight, we have another movement towards the earth; the ball is under the influence of two forces, one the explosive action of the powder, and the other that of gravitation. The resultant action of these two forces produces a curved path. The body may thus be said to be moving in two directions at the same time; a more simple illustration of space measured by two dimensions is that of a thin sheet of paper whose boundaries are the dimensions which we name length and breadth. When, however, we apply this method to substances of a different form, such as a book or a box, we find that we require to indicate a third dimension to complete our definition of such an object. In such a case we must employ three dimensions, viz., what is called length, breadth, and thickness. We have thus distance, extension, and volume—the latter being our ultimate conception of space.

* I intentionally use this phraseology.

5. *Time.*—Our usual idea of time is duration, and is a relation of the past and present, a something which, constantly changing, yet links actions which are remote with others which are near. In this way we set ourselves to adopt measures for these relations, and these measures serve for fixing definite points in this succession or flow of events which we call time. Time may well be considered as flowing onwards; as a stream whose current is bearing us onward. To note more correctly this flowing current we have devised instruments, such as clocks and chronometers, whose motions are intended to be uniform, and which in this way measure out quantities of time. These time-measurers again have to be regulated by the periodic motion of the heavenly bodies, so that as near an approach as possible to a true record of the flow of time may be made.

Time, like space in its entirety, is beyond our conception; individually we are only cognizant of limited portions of both, and even when we look backward along the stream of events until we reach the first dim life portraits of history, we have only got but as it were a scale of historic time suitable for measuring recent periods of the world's history, but which sinks into insignificance with the vaster periods of geological time, the records of which on the rock-bound volume of the earth's crust are all too dim and indistinct to give us a definite measure for our guidance.

By means of correct time-measurers we arrive at some idea of space: thus, if we measure time, as by a clock or watch, in hours, minutes, and seconds, we may speak of passing from one point to another in such a time, but if we pass over the same distance in less time the separation seems to us less, and thus if this change became very rapid we might speak of distance being annihilated.

We can measure space by time if we consider three points, two of which lie at different distances from the third. We find that a longer time is required to reach the one than to reach the other, and we might say that one point was an hour distant, while the other was two hours distant. Here we have change of position estimated by time; but as this change of position implies motion, our measurement is not complete until we know something of the relation of some measured quantity of space with a known quantity of time, which latter relation is known as velocity; or

$$s = v\,t, \text{ and } v = \frac{s}{t},$$

where s = space in feet,
t = time in seconds,
and v = velocity in feet per second.

6. *Velocity*, then, signifies the rate at which change of position is made. Thus, when we speak of passing over so many feet of space in a second of time, we call this rate of change the velocity of the moving body during the second. Here, then, we have two motions as it were, the motion of the body in space whereby it changes its position, but during this operation some other motion is going on, namely, the flow of time. Thus the body may move through 1 ft. of space in one second of time, and again it might move through 2 ft. of space in the same time. If the space, on the other hand, gets less and less, whilst the time remains the same, we gradually arrive at a point where the velocity, becoming zero, or nothing, we have a cessation of motion in space, and we say the body is at rest. If we travel by a carriage over a certain distance during a certain time, we have changed our position in space and also changed our position in time. If, however, we remain at rest for the same period of time we have not altered our position in space, but simply that in time. Time then bears us onward with a steady flow, and is unaffected by motions in space.

7. *Momentum.*—Having now considered matter and force by themselves, we come now to a consideration of matter under the action of force and having acquired velocity, and here we may revert to the original illustration of the falling stone. The stone or portion of matter, if supported at a height, would produce by its weight, or tendency to the earth's surface, a pressure upon the support, and if this pressure be effectually resisted by the support of the hand or otherwise, the body would be kept in this position and no motion would ensue. If, however, we remove the support, the stone, through the action of the now unbalanced force, will begin to fall, and will do so at an ever increasing velocity until brought to rest. During the fall the stone was impelled downward, and the impulse so induced is measured by the time during which the force acts. If we increase the size of the body we find that we have to exert more force to keep it from falling, so that for a body of double the size or mass we require twice the supporting force, and for a body treble the size thrice the supporting force, and so on. The momentum of a body is the product of the mass and the velocity. A body when falling freely in Britain attains at the end of the first second a velocity of about 32·2 ft. per second, and in gravitation results the expression for momentum is $\frac{W\,v}{g}$ where W = weight of body, v = velocity in feet per second, and g = 32·2, and it is usual to state the relation of momentum to impulse by the equation $F\,t = \frac{W\,v}{g}$, F being the force acting, and t the time of action in seconds on the body.

8. *Energy.*—This term is used to denote the capacity for overcoming resistance or doing *work*, work being an action whereby a material change is produced. Thus, when we raise the stone from above to a higher position we overcome the resistance due to its gravity, or tendency to fall to the earth's surface, and this resistance is overcome through a certain space, equivalent to mechanical work performed. The energy, therefore, which we have expended is equal to the work performed, and is measured by the weight multiplied by the height through which the body has been raised, and this energy is stored for the time in the body by virtue of the position it has now attained. If the body be allowed to fall, and return to the point from which it started, it will communicate this stored up energy to any obstacle which brings it to rest. The energy of position is sometimes called potential energy, and the energy of motion, actual or kinetic energy.

It will be seen from these illustrations that these are interchangeable, whatever is lost by the one being gained by the other. If the stone be of the weight of one pound, and be lifted through 10 ft., we have in this new position ten foot pounds of potential energy. If now it is allowed to fall and stopped at one-half of the height, or 5 ft., it has now in virtue of its position only five foot pounds of potential energy, but it has, in striking the obstacle which brought it to rest, given out the other five foot pounds of energy as actual energy of motion, hence the sum of the potential and actual energies remains constant. Although various transformations of energy are constantly taking place, yet the sum of all the energy of the universe remains constant—a principle known as the conservation of energy.

In a steam engine the energy of combustion due to the combination of the carbon of the coal and the oxygen of the air appears as heat, which is thus a form of energy. The heat through the medium of the water in the boiler produces steam, which communicates motion to the machinery. The potential energy of the gunpowder in a loaded gun becomes changed into the kinetic energy of the projectile through the chemical affinity of the constituents of the powder upon ignition. And the dull and apparently inert mass becomes suddenly changed into a large volume of gaseous matter whose elastic force propels the bullet.

In a railway train moving with a uniform speed the energy of the steam in the engine is expended in overcoming the frictional, tractive, and other resistances; if by the action of the brakes or through a collision the train be brought to rest, the actual or kinetic energy of the moving mass of the train will be mainly changed into heat of friction and in overcoming the resistances opposed in breaking up the parts in contact.

In many ways we may observe this mutual relation of heat and mechanical work, and it has been proved experimentally that 772 foot-pounds of work has to be expended in order to raise the temperature of one pound of water at 39 deg Fahr. through one degree; the number 772 is thus known as the mechanical equivalent of heat.

From various considerations it would appear that the energy of the universe is tending to the condition of heat, and that consequently there may come a period in which all matter will be at a uniform temperature with a consequent cessation of visible motion—to this principle the term dissipation of energy has been given.—*Engineering.*

THE HARMONIC ANALYZER.

IN February, 1876, Sir William Thomson submitted to the Royal Society a proposal for an integrating machine which should produce *mechanically* the harmonic constituents of meteorological, tidal, and other curves, thus obviating the large amount of work involved in their calculation by the ordinary methods. The machine thus proposed was to consist primarily of a series (connected by suitable mechanical means) of his brother Professor James Thomson's integrators, in which the motion of a disk is conveyed through the medium of a loose sphere sufficiently heavy to give the necessary frictional adhesion to a cylinder fixed in front of the disk parallel to its face. The annexed Figs. 2 and 3 will make the arrangement clear, the plane of the disk, D, being inclined at an angle of 45 deg. to a perpendicular plane passing through the axis of the cylinder, C; the ball, B, rests both upon the face of the disk and the surface of the cylinder, free to move to and fro, the points of rolling contact of the ball with the disk and cylinder describing straight lines, respectively passing through the center of the disk and parallel to the axis of the cylinder. In the integration of $y\,dx$ the distance from the center of the disk of that point upon its face on which the ball rests in its longitudinal motion represents y; while the angular space turned through by the disk from some initial position represents x. The circumference of the recording cylinder is divided into equal divisions, say from 0 to 100, by which the angular space through which it turns can easily be read off, and this reading, multiplied by a suitable numerical factor, expresses the integral in terms of any required unit.

Professor James Thomson has pointed out that an element of imperfection common to previous integrating instruments, and "which cannot but prevent our contemplating them with full satisfaction," is to be found in "the *sliding* action which the edge-wheel or roller is required to take in conjunction with its rolling action," and for the arrangement just described he claims the merit of working by pure rolling contact, instead of by a combination of rolling and slipping.

A model of Sir William's proposed machine was subsequently constructed and exhibited to the Royal Society in 1878. Five of the integrators were placed side by side, so that the centers of all the disks were in one line, and the axes of all the cylinders in a line parallel to it. A long wooden rod, supported above the integrators by guides, and worked by a rack and pinion, carried five forks acting on the spheres for the purpose of moving them to and fro; and there was also fitted to this bar a pointer for tracing the curve to be analyzed.

The curve was passed over a cylinder placed between the first pair of integrators, and turned by a handle controlled by the left hand of the operator, whose right hand governed at the same time the pointer just referred to as fixed to the horizontal bar. At the ends of the cylinder were cranks at right angles to one another, and a similar pair fitted to an upright between the third and fourth disks were set in motion by means of toothed wheels connected with a horizontal shaft at the rear of the machine; one end of this shaft was geared to a toothed wheel fixed to the record cylinder, by which it was turned, while at its other extremity it carried a large wheel with oblique teeth working into a crown wheel, and so, giving motion to the fifth disk, recording the mean. Each of these four cranks gave a simple, harmonic angular motion to one of the four disks by means of a slide and crosshead, carrying a rack working into a sector attached to the rim of the disk. The angular velocity of the second pair of cranks was double that of the first, so that the machine was fitted for analyzing meteorological curves and producing the constants in Bessel's formula:

$$E = a + a_1 \cos.\,\theta + b_1 \sin.\,\theta + a_2 \cos.\,2\theta + b_2 \sin.\,2\theta + \text{, etc.,}$$

And it was therefore handed over to the Meteorological Office, where, under the superintendence of Mr. R. H. Scott, F.R.S., its practical working was tested upon the curves of temperature and barometric pressure supplied by the self-recording instruments at the observatories of the Meteorological Council, and with very satisfactory results.

The Council of the Meteorological Office having thus satisfied themselves of its practical value, eventually requested Mr. R. W. Munro, of Gray's Inn road, to construct a larger machine containing seven disks—one for the mean and three pairs for the first three terms of the expansion—in which they introduced several mechanical improvements.

This has now been completed, and is shown in the accompanying engraving. In principle it is, of course, precisely similar to its predecessor—differing from it only in constructive details intended to secure stability and accuracy in its movements. Instead of being largely made of wood, as was the case with the model, it is entirely of metal; the cast-iron frames carrying the disks being secured to a firm iron bed supported by two substantial uprights; the disks themselves are of gun-metal, and the spheres of steel, carefully turned and nickel-plated, to prevent rusting; the horizontal bar carrying the forks for moving the spheres is also of steel and plated, and is carried above the disks upon five iron uprights or guides. The forks are provided with adjusting screws allowing of very accurate centering of the spheres upon the disks, and adjusting screws are likewise provided for the frames carrying the recording cylinders, by which their parallelism to the faces of the disks can be rigidly secured. The spheres are not touched by the forks themselves, but by the flat faces of two screws passing through their lower extremities, and in this way a ready means of preventing looseness or tightness of the spheres in the forks is provided.

Each recording cylinder has attached to it a counter for registering the number of its complete revolutions, and to secure a maximum of freedom in their movements the spindles of the cylinders, as well as the slides carrying the racks for giving motion to the disks, and the horizontal steel bar, are all made to run upon friction rollers; the slides have in addition counterpoises attached to them to prevent error from backlash.

The motion of the shaft at the rear of the machine is communicated to the second and third pairs of cranks through sets of toothed wheels so arranged that they may, if desired, be changed for others of different ratios to the cylinder carrying the curves, and in this way the terms of other orders of the expansion may be obtained, should they be required, with the same instrument merely going over the curves afresh, and using wheels of the proper ratios in place of those used for the first, second, and third pairs of terms.

The cylinders over which the curves have to pass is provided with an ingenious arrangement by which it can be expanded or contracted to alter its circumferential measurement, so that within certain limits variations in the length of the time ordinates of the curves can be very readily allowed for. To effect this, the cylinder (see Figs. 4, 5, and 6) is made in three sections, each provided with an eccentric movement; of course, except when these are at their normal positions the "cylinder" is not cylindrical, but still, even when moved to their extreme limits, the deviation is not so large as to cause any inconvenience in its use. Adjustments are also supplied to the pointers, of which two are used at once, to meet the peculiarities of the curves submitted to analysis; and throughout the machine all racks and toothed wheels are skew cut to further lessen the risk of error from backlash.

The height of the machine is 3 feet 8 inches, and the length of the steel bar, which is rather longer than the bed of the machine, 9 feet. The machine is now upon its trial at the Meteorological Office, and, notwithstanding the weight and solidity of some of its parts, the whole is so nicely balanced and fitted that it works with the utmost ease and smoothness, and in the range and accuracy of its work it will without doubt prove greatly superior to its cruder progenitors. Its novelty and the success which has attended its preliminary trials have attracted to it considerable attention from scientific men interested in such machines, and at the same time the very careful workmanship and finish bestowed upon it, and the ingenuity displayed in the adaptation of means to ends, reflects the greatest credit upon its constructor, Mr. R. W. Munro.—*Engineering.*

THE JORDAN BAROMETER.

IT is impossible to foresee the result of the perfecting of any scientific instrument. It is rarely the case that such an instrument is—like the spiders among *insecta* and their allies—born in the form which it will maintain through life. Such, however, was the case with the weather-glass of Pascal. The best mercurial barometers of the present day are in principle, and almost in detail, identical with the tube which that great genius carried up the Puy de Dome for his memorable experiment. The great defect of the mercurial barometer is the narrow limit of its movement. A difference of ten per cent in the atmospheric pressure, which is about the maximum to be obtained at any fixed point on the surface of the earth, only causes a difference of three inches in height of the column of mercury, and it is difficult to read the barometer more closely than to the tenth of an inch. The ball like, or the cup-like, form of the top of the mercury gives some indication as to the actually upward or downward tendency of the barometric movement; but the desirability of a more visible display of the force of atmospheric pressure is undeniable.

Extreme delicacy, no doubt, is obtained by the use of an aneroid, furnished with vernier and magnifying glass. But the aneroid is better suited for observations involving change of place than for those made successively on the same spot. Like the old-fashioned wheel barometer, the aneroid usually requires a tap, to see that the needle is freely moving; and this is but a clumsy means of making a delicate observation. Years ago we remember, in the window of an optician in Liverpool, an inclined barometer. The upper three inches or four inches of vertical tube were replaced by five feet or six feet of inclined tube, so that by placing the graduation at right angles to the latter, the intervals of change were magnified from twelve to fifteen times. We are not aware how the ingenious modification acted. Those of our readers who were at the meeting of the British Association in Birmingham, some fourteen years ago, may remember that one of the lions of the place was a water barometer, erected in the house of an amateur, which seemed almost to breathe like a living being. A water barometer was made by Professor Daniell for the Royal Society in 1830. The indications given by this instrument (the movement being about twelve times as great as that of the mercurial column) are very distinct. But the variation of temperature exerts such an influence on the vapor rising from the water within the tube as to interfere with the accuracy of the instrument. In fact, it is not a barometer, pure and simple, but a combination of barometer and thermometer.

Mr. Jordan, of the Museum of Practical Geology in Jermyn street, has met, and, it seems safe to say, has overcome this defect in the water barometer. He has substituted for water, pure glycerine, the specific gravity of which is 1·26—or only one fourth more than that of water, and glycerine possesses the unusual advantage of combining perfect fluidity with a boiling point of 440° Fahr., and a correspondingly low freezing point. Thus, it raises no vapor to be affected by heat in the top of the tube, nor will it be injuriously affected by frost. The only defect of the substance is the readiness with which it absorbs water from the atmosphere. This has been met by Mr. Jordan, by the ingenious device of giving the glycerine a waterproof coat, or, in other words, floating a layer of petroleum on the surface of the glycerine in the cistern. Thus, a barometer of extreme delicacy, and as little liable to get out of order as the mercurial barometer itself, has been placed at the service of science.

One of these barometers has been constructed at Kew Observatory; a second at the Museum of Practical Geology in Jermyn street; a third at the South Kensington Museum, and a fourth at the office of the *Times* newspaper. This journal has commenced the publication of two hours' reading of the instrument, plotted on the natural scale. The un-

FIG. 1.

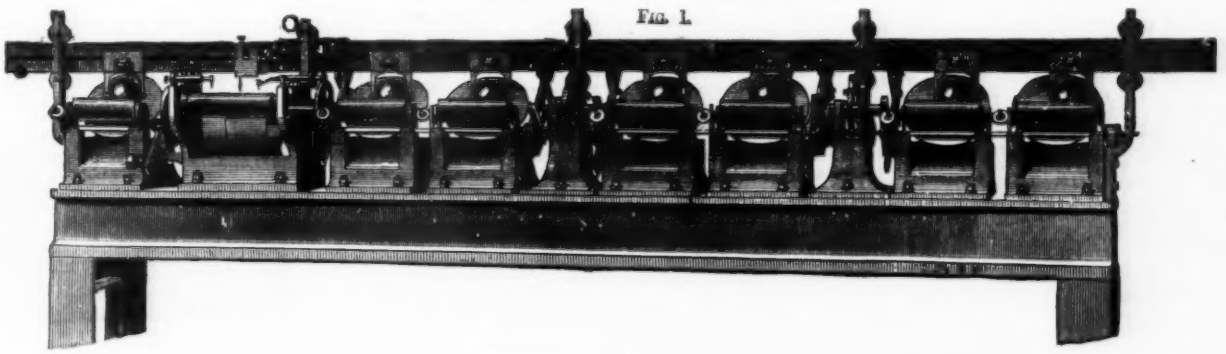


FIG. 2.

FIG. 3.

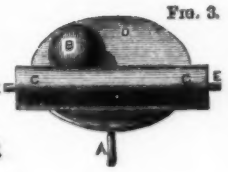


FIG. 4.

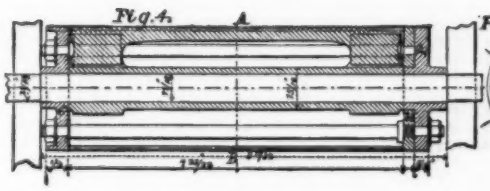


FIG. 5.

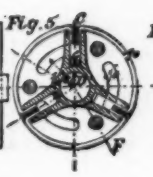
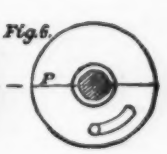


FIG. 6.



SIR WILLIAM THOMSON'S HARMONIC ANALYZER.

usual weather which has prevailed during the first week of these graphic indications has been illustrated by the very visible movement of the surface of the glycerine. What is now most to be desired is the erection of an adequate number of these instruments at certain well selected points in the island, and the publication of the contemporaneous movements of the column. The strong accentuation of the variations, already published, is such as to raise the hope that much valuable information may be secured by the proper multiplication of these delicate observations.—*Builder.*

GLASS AS AN OBSTRUCTOR OF LIGHT.

MR. F. W. HARTLEY has lately published in the *Journal of Gaslighting* an account of his extensive observations on the above subject. He says: The broad general conclusions which I deduce from my experiments are as follows, and if some of them appear to be mere truisms, I think that character will cease to be manifest if the reader will, while reading, refer to the various tables of results which I have given, the summary being only a general index to those tables:

HORIZONTAL LIGHTING.

Sheet Glass.

1. That ordinary sheet glass, apart from thickness, varies in its obstructive power to the passage of light. That the *percentage loss increases* with the distance of the glass from the flame, and *increases* also as the *light grows* stronger.

2. That ground sheet glass, apart from thickness, also varies in obstructive power. That the *percentage loss increases* with the distance of the glass from the flame, and *decreases* as the *light grows stronger*. That the percentage loss depends on which side, clear or ground, is presented to the flame.

3. That with flashed opal the losses follow the same law as ground glass for distance from, and for power of light.

4. That with clear glass as an *obstructor* of light in front of the flame, and clear glass behind the flame as a *reflector* of light, the reflected light reduces the *loss* to a degree dependent on the distance of each glass from the flame.

Globes.

5. That a clear glass globe obstructs light from an *Argand* flame, but increases the sensible light from a *flat* flame.

6. That globes of ground glass obstruct less light than sheets of ground glass. That the percentage loss *diminishes* as the light grows *stronger;* and is, for an average light, from 18 to 20 per cent.

7. That opal globes obstruct an amount of light equal to 38 to 65 per cent.

OVERHEAD LIGHTING.

8. *That the amount of light yielded by a flame in an angular direction is much less than it yields in a horizontal direction.*

9. That glass globes with elevated or overhead Argand flames *reduce* the power of the light: clear globes, about 3 per cent.; ground globes, about 21 per cent.; and albatrine globes, about 23 per cent.

10. That glass globes with flat-flame burners, at a certain elevation and within a certain radius, *increase* the power of the light: clear globes, about 6 per cent.; ground globes, about 9 per cent.; albatrine globes, about 23 per cent.; and German opal globes, about 21 per cent.

11. That reflectors greatly increase the power of the light, within a radius dependent on the shape and size of the reflector; the range in the experiments being from 52 to 92 per cent.

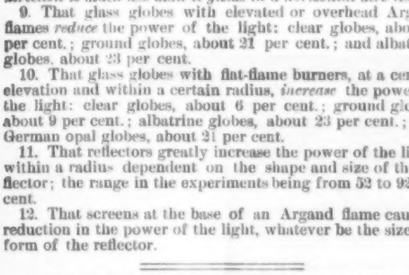
12. That screens at the base of an Argand flame cause a reduction in the power of the light, whatever be the size and form of the reflector.

A SIMPLE TRANSMISSION DYNAMOMETER.

By ELIHU THOMSON.

WHILE engaged in the work of constructing and testing dynamo-electric machines of various sizes and capacities, the need was felt of a simple instrument, by means of which a measurement of power transmitted to a machine could be readily obtained. To supply this need, the dynamometer which forms the subject of this paper was devised. It is a modification of the dynamometer which, I believe, was invented by Herr von Hefner-Altenech, and used by Dr. J. Hopkinson in his tests of Siemens' dynamo-electric machines.

Fig. 1 represents a side elevation of the essential parts and

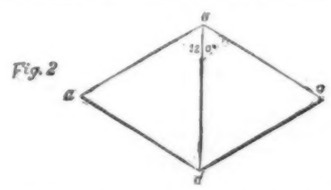


their relations to one another. D is the driving pulley, from which power is conveyed to the driven pulley, M, by a belt. The pulley, M, in my experiments, was upon the shaft of the dynamo-electric machine which consumed the transmitted power. The relative sizes of the pulleys, D and M, is immaterial. Between them, and distant from D its own diameter, is mounted an idle pulley, I, of the same diameter as that of D. Between the driving pulley, D, and the idle pulley, I, are two small idle pulleys, B C, mounted to run freely in a movable frame, F. The pulleys, B C, are placed at such a distance apart that the angles, A A, formed by lines of the belt leaving D and I, shall be each 60°. The frame, F, supporting the pulleys, B C, is hung upon one extremity of a horizontal lever, L, whose fulcrum is at H. The frame, F, and pulleys, B C, are free to move in a vertical direction through a small range, being suitably guided, as at S. The lever, L, is provided with a sliding weight, W, and has divisions marked upon it equal to, or fractions of, the distance from the fulcrum, H, to the points of attachment of the frame, F, to the lever, L.

The weight, W, is first removed, and the lever, L, and counterpoise, V, made to exactly balance the frame, F. This adjustment is made when the parts are free to move, and once made need not be repeated. It matters not what may be the tension of the belt used, the equilibrium will remain undisturbed, because the strains are counterbalanced upon the pulleys, B and C. Let now rotary motion be imparted, as shown by the arrows, and all parts be assumed to run without friction or resistance; the equilibrium of the lever and frame will not be disturbed. Further, let there be a consumption of power in turning the pulley, M. In this case the lower side of the belt will become more tense than the upper side, and in consequence, the pulley, B, having to support this added tension, and C being relieved of part of the tension, there results a downward tendency of the pulley, B, followed by C and the frame, F. The lever, L, is consequently thrown out of equilibrium. By placing the weight, W, upon the lever, L, so as to restore the equilibrium, we will be able to obtain an exact measure, in pounds, of the difference of tension of the two sides of the belt, which, multiplied by the belt speed in feet per minute, gives the power in foot pounds per minute.

Thus, if the weight, W, be thirty-three pounds, and to produce equilibrium, it is required to be placed upon the lever, L, at a distance from H equal to that of the frame, F, on the other side, then the tendency of the frame, F, to move downward is thirty-three pounds. But when the angles, A A, equal 60° each, the downward tendency of the frame, F, equals the difference of tension of the two sides of the belt, or the difference in longitudinal strains of the driving side and loose side. To determine the belt speed per minute it is, of course, only necessary to know the circumference in feet and number of revolutions of either of the pulleys, D, I or M, and multiply these quantities together. In the case assumed let the product, or belt speed, be 1,000 feet, which, multiplied by the tension difference, thirty-three pounds, is 33,000, or one horse power.

Fig. 2 is intended to elucidate the effect produced upon



the frame, F, by the increase of the tension of the driving side of the belt, and to show that the downward tendency of said frame is a measure of the difference of longitudinal strain of the tight and loose sides of the belt. Since the strains upon the pulleys, B and C, due merely to the tightness of the belt, irrespectively of its function in conveying power, are equal upon each, these strains neutralize each other, and may therefore be neglected. In Fig. 2 let *a b* and *b c* represent the differences of belt strain due to power transmission, on the portions of belt upon each side of the frame, F; that is, *a b* represents the difference of strain of two portions of the belt, one passing tangentially from the pulley, D, to the pulley, C, and the other portion passing tangentially from D to B. Similarly, *b c* represents the difference of strain of those portions passing tangentially from the pulley, I, to B and C respectively. These differences of strain act upon the pulley, B, at an angle of 120° to each other, as will be seen from the direction taken by the belt. The resultant, *b d*, Fig. 2, is equal to *a b* or *b c*, and is downward. This resultant is counterbalanced by the sliding weight, W, and measured upon the lever, L, used as a steelyard.

When properly constructed, so that the parts have sufficient freedom of motion, this dynamometer is capable of showing quite small variations of power consumed, and it has the advantage that the measurements are taken directly from the driving belt and not from intervening apparatus involving considerable friction. When the upper side of the belt is the tight or driving side, suitable modifications can be made in the arrangement of parts, and will readily suggest themselves. The dynamometer which I have used has about the following dimensions: Pulley, D, = 32 inches diameter; I = 32 inches diameter; distance between D and I = 32 inches; diameter of pulleys, B and C, = 8 inches; distance from center to center of B and C = 10 inches; length of the lever, L = 80 inches; the support, or fulcrum, H, is 10 inches from the point of attachment of the frame, F, and the lever, L, is divided on the long arm into divisions of 5 inches, marked 1, 2, 3, etc.; the weight, W, is thirty-three pounds, and belt speed 2,000 feet per minute.

In this case the numbers 1, 2, 3, etc., on the lever, L, read horse powers. The belt speed, of course, varies within certain limits, and allowance is made for its variations.—*Franklin Journal.*

ON THE GRAINS OF SILICA AND MICROCOCCI OF THE ATMOSPHERE.

By Dr. T. L. PHIPSON, F.C.S., etc.

AT the period of the great debate on spontaneous generation between MM. Pasteur and Pouchet, the latter was the first to draw attention to the fact that some of the minute spherical granulations discovered by the microscope in dust deposited from the air in various regions of the globe, were essentially composed of silica. That they had often been mistaken for eggs of infusoria or for micrococci was very evident; but when the dust was submitted to complete calcination in a platinum crucible the same grains were still visible, with the same forms and dimensions as before.

I have more than once repeated this experiment of Pouchet's, but I have also made the opposite one and examined the action of heat upon micrococci, diatoms, and oscillariæ, which are supposed to contain large quantities of silica.

There is no doubt that the dust of the atmosphere reveals to the microscope, besides the larger mineral fragments mostly of an angular shape, exceedingly minute circular or spherical bodies, having often not more than 0·001 of a millimeter in diameter, and very similar in size and shape, which resist the action of a white heat in contact with the air, and that of strong hydrochloric acid. In some of my observations they were remarkably numerous. Both before and after the action of heat they are more or less transparent. What can be the origin of these singular objects?

The same experiments repeated with silicious algæ, such as those belonging to the large family of the *diatomaceæ*, and with the *micrococci* of impure waters or vegetable infusions, showed me that they do not retained their forms after being subjected to the above treatment, and that in many instances they can be totally destroyed by heat on the object glass itself. On the other hand, the fossil diatoms resisted the action of heat and acids and retain their forms. I can only draw one conclusion from these observations: namely, that the minute silicious bodies found in the atmosphere are also fossil—they are micrococci of another age.—*Chemical News.*

CHEMICAL DECOMPOSITION INCITED BY A COLD FLUID STRATUM FLOATING ON A WARM LIQUID.*

By HENRY A. MOTT, JR., Ph.D., F.C.S.

IT seems at first thought rather paradoxical to talk of a cold fluid stratum of a given solution floating on the surface of the same solution of many degrees higher temperature, but after the momentary shock of the paradox (if a proper explanation can be given) no harm is done.

It is my object, therefore, this evening, to explain this apparent paradox, which has occupied the attention of some of our most distinguished scientists.

Dr. Antizell, the well known patent examiner at Washington, investigated this problem with great care, and frankly acknowledged his inability to explain the phenomenon it presented.

The phenomenon takes place when zincum chloride is manufactured by a new process discovered by Prof. E. J. Mallet, Jr., which I will briefly describe, and which, I believe, I have the honor of presenting for the first time to a scientific association; and then I will explain the cause of the metathetical change, the cause of the temperature of the body of the solution remaining the same, the cause of the cold fluid stratum floating on the surface of the warm solution, and, finally, wherein lies the industrial value of the process.

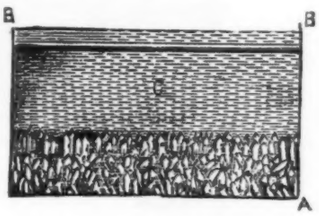
For the production of zincum chloride by Mallet's process a tank or vat is required, of suitable dimensions, having a depth of from six inches or as many feet, in which is introduced a solution of zincic sulphate and sodium chloride in equivalent proportions, having a specific gravity of about 1·25. The tank is completely filled with the solution, or filled sufficiently to cover to about one inch a horizontal connected series of pipes forming a layer of metallic tubing or piping placed near the top of the tank, the piping having about one inch or one inch and a half external diameter. The connected pipes of the series, being about one inch under the surface of the solution, are placed close together, and extend practically throughout the area of the surface stratum, and they communicate with a reservoir containing a refrigerating liquid, which by means of a force pump is driven through the piping. This current through the pipes rapidly reduces the temperature of the stratum to a depth of about a quarter of an inch to an inch, depending upon the diameter of the pipes.

The temperature of this stratum above and below the piping is reduced below 32° F., or 0° C., while the main body of the solution below the stratum remains practically at the normal temperature.

The cooled stratum at once incites crystallization, and sodic sulphate, containing ten molecules of water, separates out at the bottom of the tank, leaving a mother liquor, containing zincum chloride, which can be siphoned or drawn off and treated by the usual process.

It takes only about 45 minutes for the sodic sulphate to crystallize out of the solution, when the operation is complete.

In the figure, C represents the solution of zincic sulphate and sodium chloride. A represents the crystals of sodic sulphate. B represents the coil of pipes, as also the cold fluid stratum. The accompanying figure illustrates the tank.



The cause of the metathetical change is a problem of considerable interest, and I think few are acquainted with the fact that such changes can take place.

The changes we are to consider here, at first, are those which involve decomposition and combination without separation, and then those which involve separation of crystalline compounds.

When two saline salts are dissolved in water, a definite amount of metathesis seems always to take place, produced by the mutual interchange of their positive and negative radicals; but there is in ordinary cases no proof that any such change takes place; still it is a generally conceded fact that a mixture of four different salts is produced—the proportions depending on the relative strength of their affinities and on the quantities of each present.

A mixture of magnesic sulphate and sodium chloride in solution exists as sodic sulphate, magnesic sulphate, sodium chloride, and magnesium chloride:

$$2MgSO_4 + 3NaCl + Aq = MgSO_4 + Na_2SO_4 + MgCl_2 + NaCl$$

I suppose this change is allowed to take place because the force of cohesion among the component particles of the bodies dissolved is balanced by their adhesion to those of the liquid, and the particles of substances in solution, being free to move in any direction, easily obey the force of chemical attraction, which, of course, is a molecular force exerting itself when the particles of bodies are within distances indefinitely small.

Besides the reasons just mentioned, differences of solubility, at different temperatures, can produce alternate decomposition or metathetical changes.

In the case of magnesic sulphate and sodium chloride, of the four salts it forms, sodium chloride is the least soluble at the boiling point; by concentration (by ebullition) sodium chloride crystallizes out, and as the liquid cools magnesic sulphate crystallizes out. But if the solution is allowed to evaporate spontaneously in the open air at low temperature, sodic sulphate, being the least soluble, crystallizes out, while the readily soluble magnesium chloride remains in solution.

It has also been found† that when a solution containing equivalent quantities of zincic sulphate and sodium chloride is cooled, zincosodic sulphate crystallizes out at +10° C.; but at 0° pure sodic sulphate separates while the soluble zincic chloride remains in solution.

Malaguti‡ has also shown that a metathetical change takes place when one equivalent of zincum chloride in an aqueous solution is mixed with a solution of an equivalent

* Read before the New York Academy of Sciences.

† "Watts' Dic. Chem.," vol. v. p. 1069, 1869.

‡ "Ann. Ch. et Phys.," 1853 (3), 37, 203.

of potassic sulphate; 0·290 of it is decomposed to zincic sulphate, which may be precipitated by adding alcohol, while 0·71 of it remains unchanged. It therefore follows that when zincic sulphate and sodium chloride are dissolved in water together, the solution contains a mixture of zincic sulphate, sodic sulphate, zincum chloride, and sodium chloride:

$$2ZnSO_4+3NaCl+Aq$$
$$=ZnSO_4+Na_2SO_4+ZnCl_2+NaCl+Aq$$

If I represent the production of zincum chloride as manufactured by Mallet's process by the usual formula, we will have:

At. Wt. 64·9 31·98, 15·96 — 22·99 35·37 (Latest determ.)

$$\underset{64\cdot9\ 95\cdot68}{Zn''(SO_4)''} + \underset{45\cdot98\ 70\cdot74}{2Na'Cl'} = \underset{64\cdot9\ 70\cdot74}{Zn''Cl_2'} + \underset{45\cdot98\ 95\cdot82}{Na_2'(SO_4)''}$$

$$160\cdot72 + 116\cdot72 = 135\cdot64 + 141\cdot80$$

$$277\cdot44 = 277\cdot44$$

From this formula it will be seen that the metathetical change, while depending somewhat on the relative strength of the affinities of the radicals, and also on the quantities of each present, depends more in Mallet's process on the fact that differences of solubility at different temperatures can produce a metathetical change.

One fact will be noticed, however, that on the left hand side of the equation we find the less positive zinc (in comparison to sodium) united to the more negative sulphur and oxygen (in comparison to chlorine); but on the other side of the equation we observe that the change which has taken place has united the more positive element to the more negative elements, and the less positive element to the less negative element. In other words, we find the more positive metal sodium united to the more negative sulphur and oxygen, while the less positive zinc is united to the less negative chlorine.

It will be necessary now to consider the separation of the crystalline compound, and to do this we must inquire somewhat into the peculiarities of the various salts which are contained in solution.

Zincic sulphate is soluble in water, 100 parts of which dissolves:

At	0° C.	20°	50°	75° C
$ZnSO_4$	41·3	53	66·9	80·4 parts.

Zincic chloride is very soluble in water. It is a deliquescent salt. It is soluble in 0·333 part of water at 18·75° C. The saturated solution at 12·5° C. contains 78·5 per cent. $ZnCl_2$, at 19·5° C. it contains 92·4 per cent. $ZnCl_2$. The aqueous solution is strongly acid; its dissolution in water is attended with the development of 15,630 units of heat per molecule.

Sodium chloride is not equally soluble in cold and hot water, as was formerly supposed. According to Poggiale,* 100 parts of water dissolve

At	0° C.	5°	14°	25°	40°
NaCl	35·52	35·63	35·87	36·13	36·64
At	60° C.	80°	100°	109·7°	
NaCl	37·25	38·22	39·16	40·35	

When the temperature of a saturated solution of sodium chloride is lowered to —10° C., a crystalline hydrate separates out,† having the composition $NaCl,2H_2O$.

When the solution is further cooled to —22° C., acicular bundles of crystals separate out;‡ they are cryohydrates, $NaCl,10H_2O$.

Sodic sulphate crystallizes in mono clinic prisms having the composition of $Na_2SO_4,10H_2O$, or in rhombic crystals, having the composition $Na_2SO_4,7H_2O$.

According to Loewy, sodic sulphate can dissolve in three conditions in water, either as the deca or hepta hydrate, or as the anhydrous salt. 100 parts of water dissolve of the anhydrous salt:

At	0° C.	10°	20°	30°	34°	40°
Na_2SO_4	5·02 parts,	9	19·40	40	55	48·8
At	50° C.	60°	70°	80°	90°	100° 103·50
Na_2SO_4	46·7 parts.	45·3	44·4	43·7	43·1	42·5 42·2

Thus sodic sulphate forms one of the most remarkable cases of solubility. I have prepared a chart showing the curve of its solubility, as also the curve of the other salts we have examined, according to the latest results that have been obtained. The curve of solubility of sodic sulphate rises rapidly from 0° C to 34°, but from this point the solubility again diminishes, and at higher temperatures is seen to be still less. It is, therefore, in contradistinction to most salts, more soluble at low than at high temperatures. Sodic sulphate crystallizes from water at temperatures above 40° C. in the anhydrous state. But at the ordinary temperature of the air, the solution deposits crystals which contain ten molecules of water with one of the salt ($Na_2SO_4,10H_2O$), while at —7° the crystals formed contain, according to Guthrie, 166 molecules of water (Na_2SO_4, $166H_2O$). The hepta-hydrate ($Na_2SO_4,7H_2O$) crystallizes out when a supersaturated solution of the deca-hydrate is allowed to cool below 12° C., or when such a solution is *covered with a layer of warm alcohol*. When a saturated solution is allowed to cool *quietly* in closed vessel, and no crystals separate out, the liquid is said to be supersaturated, but if a crystal of the salt is introduced crystals at once separate. Viollet claims that a crystal of the salt must be introduced to induce crystallization, stirring or shaking will not accomplish it alone, and Gernez has shown that there are always crystals of the substance in the air over the solution.

The separation of crystals is always accompanied with a considerable rise of temperature.

According to Thomson's thermo-chemical investigations, the dissolution or separation of sodic sulphate from its water solutions at 18° C. is accompanied with the following results:

	Number of molecules taken up when dissolved in water.	Number of units of heat absorbed per molecule.
$Na_2SO_4,10H_2O$...	50	17,460
	100	18,130
	200	18,550
	400	18,760
	600	18,810
Na_2SO_4.....	400	60

From this it will be observed that when deca-sodic sulphate crystallizes from its solution about 18,500 units of heat per molecule is given to the solution.

* "Ann. Chem. Phys." [3], viii., 469.
† Guthrie, "Phil. Mag." [4] xlix., 9.
‡ Lowitz, "Crell. Ann.," 1793, i., 314.

Of the four salts we have just considered, zincic sulphate is the most insoluble at 0° C., and zincic chloride the most soluble at that temperature. This will be seen to be a very important factor in the explanation.

It may be stated as a fact that, on concentrating a mixed solution by evaporation, the salt which is least soluble at the particular temperature employed is that which is first formed; the case, however, in manufacturing zincum chloride by Mallet's process is somewhat different, as the separation of sodic sulphate is not accomplished by the aid of concentration or evaporation. The solution of the zincic sulphate and sodium chloride in equivalent proportions is made at the ordinary temperature, and the separation of the sodic sulphate is only incited by the cold layer floating on the surface of the solution, for it is only about 0° C. that pure sodic sulphate will crystallize out, as at +10° C. zinco-sodic sulphate crystallizes out in its stead. This has been proved by cooling the whole solution. It, therefore, follows that the separation of deca sodic sulphate in Mallet's process is incited by the cold stratum, as a warm stratum would not accomplish the result.

We have now to show why the body of the solution remains at about the normal temperature, not losing heat from the influence of the cold stratum floating on its surface. This problem, I think, can be very readily answered. As I stated before, when deca-sodic sulphate crystallizes from its solution, about 18,500 units of heat per molecule is absorbed by the solution, which is quite sufficient to keep the temperature of the body of solution below the cold layer warm, and prevent it from losing heat and thereby reducing its temperature by the influence of the cold stratum. Of course after all the sodic sulphate has crystallized out, and no further change takes place, the body of the solution not only should but does become in time of one uniform temperature.

The cause of the cold fluid stratum floating on the surface of the warm solution I have now to explain. It is a well-established fact that when the temperature of a solution is reduced, its specific gravity becomes much greater. From this, if from no other reason, should the cold layer or stratum, as soon as formed, settle down or tend to do so, until the body of the solution and itself become of a uniform temperature. This I have proved to be the case—the cold stratum which can be seen floating on the warm solution is constantly changing; new solution is continually taking its place, so long as there is sodic sulphate to crystallize out; and the reason that the body of the solution does not materially change in temperature is, because the amount of heat given out to the solution by the sodic sulphate in the act of crystallization, is just sufficient in this case to neutralize the tendency of the cold solution to reducing its temperature. That the cold stratum is constantly changing is supported by the fact that only at about 0° C. can deca-sodic sulphate be made to crystallize out; such crystallization can only be incited by reducing the whole body of the solution to 0°, or by reducing a stratum to 0° C. It is evident that if the stratum was not continually being replenished by the solution, only the sodic sulphate in the stratum could crystallize out; but this is not found to be the case—for the whole solution does, in a short time, practically surrender up all of its sodic sulphate in a crystalline form.

The cold stratum, therefore, arises from the withdrawal of heat from the solution by the refrigerating liquid, and would, in fact, not be a defined stratum, but the whole solution, if it were not for the counteracting units of heat liberated by the sodic sulphate in the act of crystallization, which are sufficient to keep the solution warm or at the normal temperature to within an inch or a fraction of an inch of the tubes, thus defining the stratum.

I have now finally to briefly consider wherein lies the industrial value of the process.

The value of the process lies in the fact that, as only part of the solution has to be cooled instead of the whole, less time is required to accomplish it as also less refrigerating material is utilized, two elements which figure importantly in the cost of manufacture. There can be no question but that the process discovered by Mallet will open a new era in the production of numerous salts which find application in commerce, and thereby reduce the present cost of manufacture.

UPON THE PRODUCTION OF OZONE BY HEATING SUBSTANCES CONTAINING OXYGEN.*

By DR. A. R. LEEDS.

IN his excellent "Historisch-kritische Studien über das Ozon," published December, 1879, Professor C. Engler enumerates among the modes of formation of ozone that of heating substances containing oxygen. This statement is founded upon the experiments published by Schönbein, as far back as 1855,† and which, although rendered of doubtful probability by the fact established by Andrews, that the decomposition of ozone into ordinary oxygen occurs at (and below) 270°, has never been formally refuted.

Schönbein was led to anticipate the results of his experiments by certain peculiar views concerning the state in which the oxygen existed in the bodies experimented upon. He thought that they contained the oxygen, either partly or altogether, in an ozonized condition, and he included in the number of such bodies the oxides and superoxides of all the noble metals, the superoxides and some acids of the common metals, and a number of the states of oxidation of non-metallic elements, *e. g.*, the peroxide of hydrogen, the various acids of nitrogen, of chlorine, bromine, and iodine, or in general, *all those substances which evolve oxygen on heating*. Furthermore, the reason, according to Schönbein, why these bodies underwent decomposition under the influence of heat, was that the ozone which they contained was partly resolved into ordinary oxygen on heating.

In the conduct of his experiments, Schönbein thought that he amply provided against the presence of oxides of nitrogen in the gas liberated on heating, by due care in the preparation of the bodies experimented upon. The peroxide of lead, for example, he prepared from minium by means of acetic acid, the mercuric oxide from mercuric chloride, the argentic oxide from the sulphate, and so on. And while he recognized that the chlorates, bromates, and iodates would evolve traces of chlorine, bromine, and iodine, he ascribed to these substances only a part of the effect produced, assuming that the rest was due to ozone set free on heating at the same time. The apparatus employed was of the simplest description, merely, in fact, a test-tube six inches in length and half an inch in breadth, in which the body was heated, and strips of moistened potassium-iodide starch paper, or paper dipped in freshly prepared guaiacum tincture held in the upper end of the tube.

Tried in this manner, all the substances experimented upon by Schönbein which gave off oxygen, gave at the same time the ozone reaction, the oxides of the noble metals, like argentic oxide, giving the reaction in the most striking manner, the peroxide of manganese very feebly.

In conclusion, Schönbein remarks that the escape of a portion of the liberated oxygen from the de-ozonizing action of heat, is a striking fact beyond his power to explain.

Apparently unaware of the extended research of Schönbein, and of the theoretical grounds upon which that research was founded, Mr. Kingzett published, in 1872, a preliminary note upon ozone,‡ in which he obtained results similar to those detailed above. After stating that the gas evolved on heating mercuric oxide, even after its passage through a considerable depth of potash solution, turned potassium-iodide-starch papers blue, and that the same effect was produced by the gas liberated from native or artificial manganic dioxide, even when the dioxide was heated to redness, Mr. Kingzett states that "in short, from every source I have tried, the oxygen produced never lacked these properties."

In his critical history of ozone, Professor Engler imputes to Mr. Kingzett the statement that the gas given off in these decompositions is ozone, but in justice to Mr. Kingzett it should be said that this is a misrepresentation, and that the latter states that he has no proof to offer that it *is* ozone.

In the same place, Professor Engler states§ that periodic acid, on heating to 130–135°, gives off ozonized oxygen, and quotes the experiments of Croft,‖ that iodic acid in the act of crystallization gives off ozone. Also the statement of Bellucci,¶ that during the concentration of the aqueous solution of iodic acid, and also during the crystallization of the sirup obtained by concentration, ozone is given off.

I have elsewhere shown that the apparent evolution of ozone from iodic acid in process of concentration and crystallization ceases when the iodic acid has been recrystallized a sufficient number of times to obtain it free from those traces of chlorine or nitrous acid, or possibly some lower oxide of iodine formed in the process of manufacture, and gradually eliminated by successive recrystallizations of the acid.

In the conduct of the experiments detailed below, two considerations were kept very prominently in view: 1st. To obtain the material experimented upon in as nearly as possible a state of purity. 2d. To absorb any gases other than ozone by washing.

To this end mercuric oxide was prepared by precipitation from the chloride by caustic potash, and washing by decantation and filtration until no traces of either potash or hydrochloric acid could be detected in the filtrate. The oxide, weighing 54 grammes after complete drying, was introduced into a piece of hard glass tubing sealed at one end, and so cautiously heated in a combustion furnace, that the liberated gas came over at the lowest temperature of evolution and very slowly. The oxygen was passed first through a flask containing strips of filter paper, moistened with a two per cent. solution of fresh unaltered resin of guaiacum in alcohol. These were gradually turned of a deep blue. The flask was then disconnected directly from the combustion tube, and a series of three Geissler absorption bulbs, each containing a very small amount of water (15 c.c.), was inserted between. After allowing the oxygen to bubble through the bulbs for six hours, fresh strips of moistened guaiacum paper placed in the flask were entirely unchanged. Upon the addition of silver nitrate to the wash-waters, they gave an unmistakable reaction for chlorine.

It follows, therefore, that it is practically impossible to wash out from the mercuric oxide prepared as above every trace of chloride, and that sufficient remains to give on heating a reaction for chlorine, or as Schönbein thought, for ozone.

Manganese dioxide was prepared by digesting the sulphate with chloride of lime. After the decomposition was complete, the precipitate was washed more than thirty times, until finally no trace of chlorine could be detected in the wash-water. The dried peroxide was then heated in the combustion furnace to such a temperature that a slow evolution of oxygen took place during eight hours. No change of the moistened guaiacum papers occurred in contact with the evolved oxygen, either before or after its passage through the Geissler bulbs, neither could any reaction for chlorine be obtained with the wash-water. It may be stated, accordingly, that artificial manganic peroxide prepared with due care gives on heating no reaction for ozone, or for any gas which could be mistaken for ozone.

Native manganic peroxide, on the contrary, heated for six hours in like manner, affected the guaiacum test when placed before the wash-bottles, but not when it was put after. In the latter case, the wash-water itself gave the reaction for chlorine.

Plumbic peroxide was prepared from the acetate by digesting with chloride of lime, and washing between thirty and forty times, or until the filtrate gave no reaction for hydrochloric acid. On cautious heating for six hours, the evolved gas affected the guaiacum when placed before the wash-water, but not when it was put after, the wash-water in the latter case giving the chlorine reaction.

Some argentic oxide was prepared from pure silver obtained by reduction from the chloride. After conversion of the silver into sulphate, the oxide was thrown down by potash and washed with boiling water until no traces of chlorine, etc., could be detected in the filtrate. The argentic oxide was then transferred to a small retort which was heated by an oil-bath. Air passed previously through a train of purifiers, was drawn through the retort in a slow stream during the heating. After leaving the retort it passed over a strip of moistened guaiacum paper, then through three Geissler bulbs containing water, and finally through a flask containing another strip of guaiacum paper. In the first experiment, the first guaiacum paper became strongly colored when the temperature of the oil-bath was raised to 190°, the second when the temperature had reached 213°.

This result made it appear probable that the air of the laboratory was not sufficiently purified, and the experiment was again repeated with outside air. Identical results were obtained, the first paper changing at 190°, the latter at somewhat higher temperature, and the heating being continued until the oil-bath had risen to 280°. An experiment was then made in blank, purified air being drawn through the train of apparatus without heating the argentic oxide, when it was found that both papers altered in contact with the air alone, though at a slower rate than in the previous experiments when the air was heated. Having established in this manner, that the change of color of the guaiacum tests was insufficient to prove the presence of ozone, they were replac-

* A paper read before the American Chemical Society.
† "Verhandl. d. naturf. Gesell. Basel, i., 352. Jour. f. prakt. Chem., lxvi., 286. Phil. Trans., 1860, 113.

* Chem. News, xxv., p. 242.
† "Historisch-kritische Studien über das Ozon," p. 26.
‡ Chem. News, xxv., 87.
§ "Ber. d. deutsch. Chem. Gesell.," 1875, viii., p. 908.

ed by zinc-iodide-starch papers. On raising the temperature of the retort containing the argentic oxide to 284°, the first iodide paper turned blue, the second, which was placed after the series of Geissler bulbs, remained entirely unaffected. The reaction was due to the presence of a trace of chlorine, but some difficulty was experienced in demonstrating this to be the fact. The wash-water gave so faint an indication, that it was not until the whole amount of wash-water in the three bulbs, amounting in all to 45 c. c., had been evaporated down to 5 c.c., that an unequivocal reaction for chlorine was obtained.

Some periodic acid was then prepared by digesting one pint of iodine with twenty pints of sodium carbonate in two hundred pints of water, and passing a stream of chlorine through the liquid until no further precipitate was formed. The sodium periodate was then filtered, slightly washed with cold water, dissolved in dilute nitric acid, and decomposed with argentic nitrate. The resulting diargentic periodate was crystallized out of its solution in hot nitric acid, decomposed with cold water, and the aqueous solution of periodic acid thus formed evaporated to dryness six or seven times, until no trace of acid fumes could be detected in the escaping moisture.

The periodic acid was heated in a retort placed in an oil-bath, a slow stream of purified air being drawn over it during the course of the operation, and at no time was the temperature allowed to rise above 154°. In the first set of trials, the guaiacum papers placed before and after the Geissler bulbs containing the water employed in washing the gas, were colored blue. After the discovery that the air alone could effect this change, zinc-iodide-starch papers were employed, when the paper placed before the wash-waters was turned blue, that placed after remained unaltered. The wash-waters gave an unmistakable reaction for chlorine.

The conclusion to be drawn from the foregoing experiments is, that the supposed evolution of ozone upon heating certain oxides of both metallic and non-metallic bodies, is in reality due to minute traces of impurities, usually chlorine, which cannot be eliminated in the ordinary course of preparation of these bodies.

ON RATIONAL SEASONING OF WOOD, WITH SPECIAL REGARD TO THE "BEERIZING" PROCESS.

By SIGISMUND BEER.

IF an inventor tells you that he has invented a new process for seasoning wood, by which the quality of wood is improved in appearance, strength, stability, and durability, and that so seasoned wood looks finer, brighter, and compacter in fiber, is of increased bearing power, and stands atmospheric vicissitudes without any change, and will neither swell nor shrink nor decay on exposure, and if you are an expert by practice concerning wood, you will feel inclined to suspect that he is either an impostor or an over-sanguine self-deceived inventor. Very likely you will not lose your time and listen to his explanation, nor trouble yourself to examine his wood, because the apparent nature of wood, which you know well from your own observation, is wholly against it. I say this in general, not from fancy, but from a ten years' experience in my intercourse with hundreds of prominent men in trade and profession. The higher they rank therein, the more intimately they feel themselves acquainted with the various kinds of wood, the more prejudiced I found them. Two instances may here be mentioned, which are significant and instructive: About ten years ago, passing the extensive well-known timber yard of Messrs. Bell, foot of Twenty-second street, North River, New York City, I saw men sawing off ends, about two feet long, from beams of spruce wood, and was allowed to take one of them. A week later, after I had seasoned it over in my way, I took it to the office, in order to find out to what extent, if at all, they would there recognize the difference in appearance and quality. Handing it to the elderly Mr. Bell, I asked his kindness to examine this piece of timber, but did say no more. He went with it near the window, and looking at it silently for some time, during which he repeatedly raised and lowered his spectacles, as if he does not trust his look, I finally interrupted his study by asking: "What is the matter, Mr. Bell?" "Oh, sir," he said, "I have seen various growths of this kind of wood during my life, but never yet saw it so fine in grain and so compact as this. Where did you get it from?" "I took it from your yard, right here outside," was my prompt answer. "You are mistaken, sir, we have no such wood in our yard." "I took it from here about a week ago." "That is a mistake, for we never had this quality of wood." "Oh, excuse me, sir, I forgot to mention that I have it in the meantime more thoroughly dried." "If you dry it a hundred years, you cannot make it like that," was his answer, spoken half angry and in a tone distinctly showing that he felt somewhat offended, just so as if suspecting that I intended to humbug him. At first perplexed by the turn things took, I soon regained composure, and tried, but in vain, to alleviate his feelings and argue matters further. I left the office deeply moved, and regretting that my anxiety to have the merits of my invention investigated without any bias by a competent judge brought me morally under a cloud in the eyes of this highly respectable citizen.

The second significant case occurred about the same time. At the office of Sol. J. Gordon, Esq., I was introduced to Mr. J. E. Braunsdorf, formerly of the firm of Weil & Braunsdorf, well known manufacturers of furniture. My invention being discussed, Mr. Braunsdorf stated that he was, since his boyhood, in the woodworking business, that he knows well the nature of wood, and that he is positive that it is impossible to season wood so that it will neither swell in moist air nor shrink in a dry atmosphere. To prove his assertion, and very probably more so to guard his friend, Mr. Gordon, who took special interest in my invention, against imposture, he proposed to select and send some wood to me, to be seasoned, and then to be tried by him, which proposal was accepted and executed. About three months later I inquired about the result of his trials, when he answered: "Mr. Beer, I must apologize. I have myself carefully tried the wood with the aid of a fitted gauge. I measured the pieces when I received them, and after I kept them for three weeks in the cellar under the moist moulding sand, and also after I kept them for three weeks over the boiler exposed to hot, dry air, and found that they did not change their volume. The result proves that you were right, but nobody will believe you; better I give you a written certificate," and so he did.

WHAT WOOD REALLY IS.

All the preoccupation of mind, as instanced above, and all other abounding prejudices and confused ideas in regard to the nature of wood and its proper treatment, find their explanation in the circumstance that the very material which we simply call "wood" is not wood alone, but a mixture of wood and sap in about equal proportions in average in fresh logs. Now, wood, so allied, is only known and so far used in trade and arts, while pure wood, such as is obtained by my process or by washing out the sap before drying, is a wholly new article. When compared, a very decided, if not surprising difference is observed between the commonly seasoned, unwashed wood, and such first washed, as well in regard to their chemical nature as to their physical properties. The chemical effect produced by mere washing will be strikingly observed by

A SIMPLE EXPERIMENT.

Take two glass jars. Into one put so much sawdust of fresh wood as to fill it up to about two-thirds. Into the other put sawdust of the same quality and quantity, which has been previously washed in cold water to remove the sap. Cork well and place the jars where they are kept at a moderate temperature of about 70°. When you, after a day or later, at any time remove the corks from the jars without shaking the contents, and you dip a lighted taper or match into the bottle containing the unwashed wood, you will at once observe that the flame extinguishes just as if put into water, but when the lighted taper is dipped into the other jar containing the washed wood, its flame continues to burn just as in the free air. You may now shake both bottles, in order to change the air therein, and replace it by fresh air from without, cork again and try the taper as before, and you may repeat the experiment over and over again, and you will always find the same result as at first.

EXPLANATION.

The fact that the flame extinguishes in the jar with the unwashed wood shows evidently that the atmospheric air therein has undergone a certain change, that its oxygen has left it and has entered in a chemical combination with the carbon of the wood, producing thereby carbonic acid gas, which possesses the characteristic property that a burning flame brought into it extinguishes as if dipped into water. In other words: the above experiment shows that wood, when allied with sap, suffers decomposition, commonly called "decay" or "rot," by the action of the air, and it shows, also, that no change takes place by the same treatment upon washed wood; or, with other words, that the wood is rendered indecayable by the removal of its sap.

Bear in mind that we selected this experiment because it is as simple as it is instructive to recognize the silent but persistent decaying process which takes place all the time, while wood with the sap left in is exposed to the air, and to demonstrate the striking difference therein by mere purification, and, also, because it affords a very useful and convenient means for testing the decayability of any kind of wood by treating its shavings, moisted with water, in the same way as prescribed for the sawdust. But bear also in mind, that the principles governing decay, as elicited thereby, have long since been fully established from most elaborate scientific researches, with the aid of exact chemical analysis and of the microscope under the most varying circumstances. Suffice it here to cite the main facts, as we find them reported by Sir Humphry Davy in his lectures held in the beginning of this century, and printed in vol. 8 of his "Collected Works:"

"If any fresh vegetable matter, which contains sugar, starch, or other vegetable compounds soluble in water, be moistened and exposed to air at a temperature from 55 to 80 degrees, oxygen will be absorbed and carbonic acid formed, heat will be produced, and elastic fluids, principally carbonic acid, gaseous oxide of carbon, and hydrocarbonate, will be evolved, a dark colored liquid will be formed, and, if the process be suffered to continue for a time sufficiently long, nothing solid will remain except earthy and solid matter colored black by charcoal."

"In proportion as there is more gluten, albumen, or matters soluble in water in the vegetable substance exposed to fermentation, so in proportion, all other circumstances being equal, will the process be more rapid.

"Woody fiber will not ferment, unless some substances are mixed with it which act the same part as the mucilage, sugar, and extractive or albuminous matters with which it is usually associated in herbs and succulent vegetables. Pure woody fiber alone undergoes a change very slowly, but its texture is broken down, and it is easily resolved into new elements when mixed with substances more liable to change, containing more oxygen and hydrogen. Volatile and fixed oils, resins, and wax are more susceptible of change than woody fiber when exposed to air and water, but much less liable than the other vegetable compounds, and even the most inflammable substances by the absorption of oxygen become gradually soluble in water."

Enlightened by such knowledge, it is easily conceived that the most perishable wood will be converted into the most durable substance merely by proper purification. Difficult as it may appear to persons less trained in exact science to comprehend this fact in its full meaning and bearing, and that, for instance, a railroad tie of cleansed wood will remain sound for hundreds of years on places where a tie of the same wood, but associated with the sap, will rot away in less than five years—those persons have to be reminded that such is the supreme law of nature, to which human logic and fancy have simply to submit. It is just from utter disregard of these laws that

ALL PREVIOUS ATTEMPTS TO PRESERVE WOOD

failed. It was first Dr. Kyan who, mistaking the growth of lichen and fungi on decayed wood for the cause of decay, patented, in 1832, an impregnation with a solution of corrosive sublimate (bichloride of mercury) for the preservation of wood, because that substance is the strongest poison to organic life. Another inventor followed by substituting chloride of zinc for the mercurial chloride, which, although poisonous, is less dangerous to handle and less costly, but overlooking thereby that chloride of zinc is in itself a destructive agent for woody fiber, and that a concentrated solution of it dissolves paper, sawdust, and like fibrous matter. Other inventors followed with nearly all the various soluble metallic salts, such as chlorides, sulphates, and acetates of copper, iron, lead, etc., for no other intelligible reason but to blindly follow the path of the inventive predecessors.

Similar is the case with

CREOSOTIZING TIMBER.

Centuries ago tarred posts have been used, and found to last under circumstances somewhat longer than others on account of the tar forming a coat upon the fiber, which impedes the access of air, but tar and the best so-called "creosote-oils" undergo themselves decay, though less rapidly than sap matters—they change from the liquid state into a more hardy, resinous state by oxidation, and then cease to be protective and become instead thereof additional instigators to decay. Hence it will not surprise that David Stevenson, the eminent English engineer, reports (*Civil Engineers' and Architects' Journal*, vol. 25, page 205, London, 1862) that he has been induced by John Bethell to try the durability of wood, creosotized by him to the fullest extent, for piers and water structures, that he did so and found such wood totally unfit for the designated purpose, and that it was soon destroyed, perforated and eaten off in places where the oily preparation was not abundant.

In all of the above processes only impregnations with foreign matters were applied, but the sap, the real cause of the decay, was left in the wood.

THE BEERIZING PROCESS

for seasoning and preserving wood consists in the use of a solution of borax in water to dissolve and extract the sap matters out of the wood.* It is evident that the value of this process depends principally on its purifying efficiency. To prove the purity of wood, chemical analysis offers the surest means. Pure wood contains no nitrogen, while nitrogen forms the most characteristic elementary constituent of sap matter. On examining beerized wood in the laboratory of the College of the City of New York by Prof. Doremus and his assistant, Dr. O. Loew, no nitrogen was found in it, while nearly two per cent. of nitrogen was found in the same wood not prepared. The beerized wood is, therefore, perfectly pure, and as such absolutely not liable to decay. Its excellent quality in that and other respects is testified by reports from the responsible officers of one of the best and most cautiously managed railroad companies in the United States, the New York, New Haven, and Hartford Railroad Co. Mr. H. Kettendorf, superintendent of motive power, under date October 14, 1880, reports: "Yesterday we examined ties and timber, which were treated and placed under ground in August, 1878, also the wood in engine cab; find all in good condition—good as when first put in."

Mr. Jas. Denver, master car builder, reports, under date November 6, 1880: "That after two years of service the ties treated remain apparently in as sound and good condition as when laid down. Pieces of such wood used otherwise remain also without change by atmospheric action, have not checked, shrunk, or swelled."

Still, indecayability is not the only superior quality of beerized wood. Its tissues being not distended by sap, water, or foreign matter, it is more compact, of increased strength and bearing power, less porous and penetrable by liquids, closer in grain, and finer in appearance. It is also vermin-proof. No albuminous sap substances, which in unwashed wood predominate, and furnish eagerly sought nourishment to insects, thereby causing their ravages, are here present.

Aside from the improved quality, which the new process yields, though such is the most important point,

ITS CHEAPNESS AND QUICKNESS

are favorable items. A perfect seasoning is effected in fewer weeks than it takes years by the old process, saving thereby interest on capital, expense for work of occasional rehearing, loss from rot, cracks, and destruction by insects, while the cost of the new seasoning is comparatively insignificant.

The process, although patented, is not confined to a monopoly. A license will freely be granted to responsible parties by application to S. J. Gordon, Esq., 44 East 14th street, New York, charging only a trifle for royalty. It is the earnest desire of the patentees to bring this useful process speedily into general use. But to accomplish such they have to struggle against and to overcome the mighty power of old custom and of indifference, of ignorance and of prejudice, which in this case is more general and obstinate, because the rationale of the innovation lies beyond the horizon of ordinary instruction and comprehension. For this reason, and because the proper seasoning and preservation of wood is a matter of highest importance to the general welfare of our country, they respectfully appeal to the press and all better educated, patriotic citizens for assistance by propagating correct views and a better understanding of the matter. To such appeal they feel especially encouraged by the emphatic call of warning against the waste of timber, issued in the official "Agricultural Report" (Washington, 1866), which reads:

"There are few subjects so closely connected with the wants of society, the general health of the people, the salubrity of our climate, the production of our soil, and the increase of our national wealth, as our forests, and yet no interest of our country has received so little attention at the hands of the people, and enjoyed so little fostering protection from the government. It is my intention in this article, by a simple array of important facts, and a few passing suggestions, to call the attention of our landlords, farmers, and mechanics to an impending national danger, beyond the power of figures to estimate, and beyond the province of words to express. If I can influence these classes but a little, if but few facts shall be added to the knowledge possessed by each, and if, therefore, but a slight effort be put forth by every one of them, the aggregate of interest, intelligence, and action will be immense.

"It is feared it will be long—perhaps a full century—before the result at which we ought to aim as a nation will be realized by our whole country, to wit: that we shall raise an adequate supply of wood and timber for all our wants. The evils which are anticipated will probably increase upon us for thirty years to come, with tenfold rapidity with which restoring or ameliorating measures shall be adopted. Every hour is precious! We have as a nation far too long disregarded this interest. Growth is slow, while destruction is rapid and instantaneous. Delay, therefore, is both cruel and disastrous to ourselves!"

ARSENICAL WALL PAPER AND ARTIFICIAL FLOWERS.

THE Boston *Globe* credits Dr. Edward S. Wood, professor of chemisty in Harvard College, with the statement that about one-third of the wall-paper examined by him has been found to contain arsenic, though no information is given as to whether the papers in question were or were not examined because they were previously suspected of possessing deleterious qualities. Dr. Wood further states that he has found by experiment that arsenic may get into wall paper in three obvious ways: First, by being applied to the paper in the form of a well-known and commonly recognized arsenical pigment, such as Paris green; second, by the use of an arsenical pigment, which is supposed to be free from arsenic, such as fuchsine and other aniline pigments; third, non-arsenical pigments may be applied to the paper by means of a glue or sizing which contains arsenic. When arsenic gets into wall paper in either of the last two named ways, it is evident that its presence is unknown to the manufacturer. The colors in which arsenic is most generally found are green, red, and blue. The light green shades are more liable to contain arsenic than those of a darker hue, which is probably due to the fact that dark green can be obtained very cheaply by mixing non-arsenical blue and yellow pigments. The red colors are fully as liable to be arsenical as the light greens, as a great many of the red pigments are made by being oxidized with arsenical acid, and a portion is retained in the color owing to improper purification. Other red pigments contain arsenical acid in their composition, and in

* Borax does not, like other solvents for albuminous matter, affect tissues.

some cases arseniate of aluminum is used as a mordant. There are a number of well authenticated reports of poisoning from the use of arsenical aniline pigments in the manufacture of wall papers, and also of artificial flowers, and in one instance where an investigation of a fatal case of arsenical poisoning of a person employed in the manufacture of artificial flowers, it was found that out of six specimens of fuchsine, as sold in the shops, only one did not contain arsenic. Expensive and high-priced wall papers contain arsenic about as frequently as the cheaper ones, and this is true of the blue shade in expensive papers. Of two specimens of a beautiful red pigment, used in the manufacture of wall paper in Germany, when examined, it was found that one contained 1·96 per cent. of white arsenic and the other 2·49 per cent. Dark slate colors and drabs, colors which would seem to be the last to be suspected, are very liable to contain this poison, and, in fact, persons do not know, without having samples of the paper with which they wish to adorn their walls examined by a chemist, whether they are placing themselves in peril of sickness or death, or not. In rooms in which green papers containing arsenic are hung, a very simple experiment will detect presence of this poison. If a handkerchief is passed across the top of the door which has been left open for any length of time, there will be seen adhering to the handkerchief green patches, which have been rubbed or in some other manner displaced from the paper, and it will be readily seen how dangerous are these papers when these poisonous particles are floating about, and are breathed into the lungs or lodged in the pores of the skin. When paper containing arsenic is put on the walls of a room the plastering will absorb a certain amount of the poison, and although the paper may be removed the walls will still contain and give out poisonous properties. A case of this kind occurred but a short time ago. A lady having become distrustful as to the paper in one of her rooms sent a sample of it to a chemist for analysis. It was sent back with the statement that it contained no arsenic. Some two weeks later he received a communication from the lady that she, being entirely reassured by his assertions, reoccupied the room after an absence of over two months, but that the symptoms of arsenical poison immediately returned to her, although they had all disappeared while she had occupied another room. They increased markedly from day to day. The chemist could not account for these facts until it was learned that the paper formerly on the wall was a brilliant green and unquestionably arsenical. This paper was the first cause of a serious illness, whose symptoms indicated that it was caused by poison of some kind. The green paper was taken off, in doing which the paper was wet with water to facilitate the work, and streaks of green were thus left on the wall. When new paper was put on the wall, as stated, the paper was subjected to the chemist and found to contain no noxious properties, but the poison which had adhered to the wall was thrown out into the atmosphere of the chamber through the new paper and operated with serious results on the person occupying the room. The amount of arsenical pigment which adhered to the wall must have been quite small, but the case only goes to show that to preserve the health of persons who are very sensitive to poisons, especial care should be taken to see that the paper employed in decorating the walls of houses is perfectly free from this class of poisons.

Persons may be differently affected when subjected to the same amount of arsenical dust or vapor. Two persons may occupy the same room or sleep in the same bed, and while one may feel the effect of the poison very quickly the other may not be affected at all, or, at any rate, for a long time. A person may work in a factory or store where there are poisonous wall papers or artificial flowers prepared with arsenic and not feel any bad effects, while another person would find it impossible to live in the tainted atmosphere without being constantly subjected to sickness. Children are a great deal more susceptible to these influences than adults, and females are more susceptible than males. Undoubtedly, arsenical wall paper produces its effects upon persons occupying rooms whose walls are hung with such paper, chiefly by means of the minute particles of poisonous dust which are set free from the paper, and coming in contact with the mucous membranes inhaled into the lungs, cause disease. In some cases sitting in a room whose walls are hung with arsenical paper has been sufficient to produce marked symptoms, while in other cases months have elapsed before any decided effect on the health has been observed. The effects produced by arsenical compounds used in wall paper are twofold, local and general. The local action is an inflammation or irritation caused by the contact of the arsenical compound with the skin or mucous membrane; the general action is due to the effect of the arsenic upon the system after it has been absorbed into the blood. The inflammatory action is most noticeable in those parts with which the dust can most easily come in contact, such as the mucous membrane of the throat, nose, and eyes. Workmen engaged in the manufacture of wall paper may frequently be seen having an eruption of the skin of the hands and arms, which is caused by the poison being retained on the flesh and worked into the pores of the skin. The symptoms of the local action of arsenical poison are usually at first those of an ordinary cold, when the mucous membrane of the throat is affected, and sometimes without a cold. The mucous membrane of the throat, nose, and eyes being more easily affected by the poison, is usually irritated or inflamed to a certain extent. The inflammation is often the most marked symptom of chronic arsenical poisoning, and differs from the ordinary inflammation which is the result of exposure to the weather by its long duration and resistance to ordinary treatment. Sometimes, if the dust is inhaled into the throat or lungs, a bronchitis is produced, and shows itself by the usual cough and expectoration. After a period of these symptoms by a continued irritation of the stomach caused by the small particles of arsenical dust swallowed with the saliva, more or less indigestion is produced, and perhaps diarrhea, caused by its action on the intestines. Chronic inflammation of the stomach and intestines will sometimes ensue from this poison. The effect of the poison is shown later, chiefly by disturbances of the nervous system, which may vary in different cases, but most frequently, perhaps, take the form of neuralgia in different parts of the body. Severe headache, great debility, emaciation, cramps, convulsions, and even paralysis have been seen as symptoms of the severe or long continued action of the arsenical emanations from wall paper. Death may finally ensue either from some intercurrent disease, which the patient is unable to withstand on account of the low condition of his entire system. If while suffering from these symptoms of arsenical poison, a change of residence or of room is effected for a short period, a marked improvement in the condition of the person takes place, while previous to such removal or change different remedies and a variety of treatment may have been used without avail, but on the return of the person to the same house or room he will immediately be attacked with his former symptoms.

The earlier and milder symptoms are so obscure and similar to those that may be due to other causes, that an accurate diagnosis is exceedingly difficult. The absence of any definite cause, such as exposure to the inclemency of the weather, or cold, etc., to account for the head and throat irritation, or symptoms in spite of treatment, the disappearance of the symptoms upon removal from a certain room, and return of the symptoms upon the return to the same room, are all important points to bear in mind in making a diagnosis. Finally, improvement and recovery after the removal of the arsenical paper from the walls of the room, will prove that the former symptoms were due to poisonous emanations from the papers. In conclusion, it would be well to say that no person can be too careful in seeing that no danger threatens himself or family by allowing them to be constantly exposed to sickness, and perhaps death, by allowing the walls of his room to be papered with this subtle and dangerous poison, and also that manufacturers should be very careful about allowing any of this poison to get into the papers which they manufacture, as it is sent out to deal disease and death among the populace at large.

[A fatal case of supposed arsenical poison occurred recently in Port Jervis, New York, the poison having been absorbed from a pair of stockings dyed brown and old gold.]

SEPARATION OF COBALT AND NICKEL.

By F REICHEL.

THE circumstance that the detection of small quantities of cobalt in presence of nickel is tedious induced the author to utilize the solubility of the oxides of cobalt in concentrated potassa lye for this purpose.

If, in the course of analysis, a precipitate is obtained which may contain cobalt and nickel, it is not possible, if the nickel preponderates, to demonstrate the presence of cobalt by a simple blowpipe experiment. It is necessary to dissolve the precipitate, to add potassium nitrate, or to precipitate cobalt, if present, as hydroxide by means of a current of chlorine, and the subsequent addition of barium carbonate—an operation which requires several hours.

The author recommends to dissolve the precipitate, to throw down both metals with potassa lye, and place the precipitate on a filter. After the liquid is run off the precipitate, without washing, is introduced into a test tube; a fragment of caustic potassa and very little water are added, and the whole is brought to a boil. The more concentrated the potassa solution the easier is the separation. The cobalt dissolves with a blue color, and even very small quantities may thus be detected. The mixture is immediately filtered through a small asbestos filter, which should be in readiness, washing first with hot concentrated potassa lye and then with water. From the filtrate the cobalt may be precipitated as hydroxide by the addition of ether.—*Zeitschrift für Analyt. Chemie.*

SUNDRY NOTES.

The Use of Sirups in Brewing.—Brewers are now allowed to use sirups at rates of duty fairly proportional to the quantity of sugar they contain. By a recent general order, 34 lb. of cane sugar sirup not exceeding a density of 14 lb. per gallon, and 41 lb. of grape sugar sirup not exceeding 13⅛ lb. per gallon, are respectively deemed equivalent to 28 lb. of sugar, and pay duty accordingly. We do not object to these equivalents, and think they will be found to work fairly toward both brewers and the revenue, and we deem the concession so far a valuable one; but we consider the duty on sugars and sirups used in brewing might be assessed on a far more rational and scientific basis. There are no practical difficulties, but such as might be easily overcome, in the way of determining the exact percentage of dry sugar in each particular sample; the relation between sugar cane and grape sugar is a definite one, and the methods of determining the percentage of sugar are exact and easy of application. If the dealer in sugar were bound to declare the true percentage of sugar in every parcel he sold to the brewer, the duty could be assessed on this, and the excise could always have the right to check the correctness of the declaration. The determination of sugar by means of Fehling's solution is easy of application, and is a method which any brewer or excise-man could learn to perform with sufficient exactness by a little practice, and there would, of course, be the appeal in any case of dispute to the Somerset House chemist. Not only might the duty of sugar used in brewing be assessed on the exact amount of dry sugar contained in any particular sample or solution, but even the duty on malt and other grain might be assessed on their yield of sugar.

The Influence of Oxygen on Different Ferments.—Those who have had any experience in the microscopical examination of yeasts and beer deposits cannot but have observed the different effects produced by exposure to the aerating influence of the atmosphere. The yeast cells are certainly not injuriously affected by atmospheric oxygen, and some close observers have even asserted that under these conditions the cells become enlarged and offer other evidence of increased vitality. It is, on the other hand, clearly established that oxygen is inimical to the life of certain parasitic organisms accompanying yeast; the various forms of vibrios are especially susceptible to the influence of aeration, and the mere exposure of a drop of liquid containing these organisms on the glass slide, preparatory to microscopical examination, is sufficient to temporarily destroy their vitality. The experienced microscopist will, therefore, take precautions in these respects, in order that his observations may lead him to correct conclusions. The teachings of scientific men have now convinced most practical brewers that the influence of oxygen on a fermenting wort is favorable, and, as a result, we find many more mechanical arrangements for rousing and aerating the worts than were in vogue years ago. In the last generation brewers looked upon air as a poison to their wort and beers; now they know it is a necessity at certain stages of the process, and a most useful stimulant to the healthy growth of the yeast plant. Science, however, goes even further: she tells brewers that if they apply atmospheric air they must take one important precaution, and that is to insure its purity, so that no diseased germs are thus introduced.

Researches on an Acidifying Ferment.—In a recent communication to the Academy of Sciences at Paris, M. Boutroux has given some most interesting information concerning a peculiar ferment which generates acid in beer wort. He first isolated this ferment by placing a small quantity of the deposit from a bottle of beer in some malt extract, and by repeated cultivation the yeast cells and other organisms were eliminated, and the ferment was obtained in a pure state. At first the ferment presented the appearance of oval or spherical cells, in some cases separate, in others grouped together in chaplets. These cells have nuclei at first, but as they develop the nuclei disappears, and the cells diminish in size. After several months the cells take the form of irregular and rather long filaments. These filaments, when sown in a suitable medium, produce a considerable quantity of acid. It was proved that these cells absorbed oxygen and evolved carbonic acid; and further, that they could not exist without oxygen, but the absence of this gas in some cases did not kill them immediately. Yeast water was found to be the most suitable cultivating medium. Cane sugar, invert sugar, and glucose are all acidified by this ferment, but the last named with the greatest facility. The temperatures most favorable to the development of this ferment are between 86° and 95° F.; above 99° F. the development takes place with difficulty, and ceases altogether at 104° F. A temperature of 131° is fatal to the old cells and 140° to the young ones. It was found that the cells would, while in an active state, retain their vitality for upwards of three years. This organism acts upon alcohol in the same manner as *Mycoderma aceti*—that is, by converting it into acetic acid, *vice versa*. M. Boutroux found that *Mycoderma aceti* converts glucose into acetic acid, but he does not consider the two organisms are identical, and he proposes to call the one, which is the subject of this note, *Micrococcus oblongus*. During the fermentation induced by it a little carbonic acid is formed, but no alcohol, no volatile acid, nor any other volatile substance; there is left a glucose which differs but slightly from ordinary fermentable glucose; the principal product is an acid, which, in fermentation made in the presence of carbonate of lime, yield crystals of the lime salt. M. Boutroux proposes to call the acid resulting from the fermentation of glucose by *Micrococcus oblongus*—zymogluconic acid, and the formation of one equivalent of this acid corresponds to the destruction of one equivalent of glucose and of two equivalents of oxygen. The fermentation induced by this ferment partakes more of the character of a combustion, and is analogous to the acetic fermentation of alcohol. This organism, instead of being anaerobic, as is the case of most ferments which give rise to the evolution of gases, is aerobic, or dependent upon a supply of air. The *Micrococcus oblongus* does not secrete any soluble ferment—it is a simple agent of oxidation capable of carrying free oxygen to glucose or alcohol, and thus bringing about a partial combustion of these bodies. These remarks of M. Boutroux are an interesting addition to our knowledge of the organisms which attack beer: Dangerous as this new organism may be, it has been pointed out that it is incapable of resisting a temperature of 140°, and it is for the practical brewer to avail himself of this knowledge if he wishes to destroy and counteract the evil effects of this ferment.

Beer in Frosty Weather.—The present inclement temperature is seriously affecting many stock beers. With the thermometer at about 15° below freezing point, the construction of ordinary beer cellars is not substantial enough to keep out the cold, and the beer becomes chilled; as most substances are less soluble at low temperatures than higher ones, and as this is especially the case with albuminous matters, we find that cold causes some of these to deposit, and the beer thus becomes very turbid. When once deposited they are dissolved again with great difficulty, and it sometimes takes several weeks of warmer weather to get the beer into condition again. Our English beers fermented at high temperatures are particularly susceptible to cold, and are quite different to the Bavarian and bottom fermentation beers, which withstand the influence of a low temperature without undergoing any appreciable change. One of the worst results of cold is to flatten the beer; in its normal state beer contains a number of ferments which gradually decompose the saccharine extract, giving rise to the evolution of carbonic acid; as soon as the beer becomes chilled these organisms either cease altogether, or, at all events, to a great extent, to perform their vital functions, and thus the generation of carbonic acid ceases. The change to a warmer temperature may restore the brilliancy of the beer, but it takes a long time for these organisms to recover their vitality, and thus a chilled beer remains flat for a long time after it has gained a normal temperature. It is, therefore, of the utmost importance to brewers to keep up the temperature of their store cellars; with this object all doorways, windows, and openings should be kept securely closed. It is seldom that we have such cold weather as we are experiencing at the present time, and our cellars are not constructed to withstand the attacks of cold; it therefore behooves our brewers to take extra precautions.—*Brewers' Guardian.*

PROGRESS IN CHEMISTRY.

A PROCESS for the manufacture of ammonia from the nitrogen of the air, invented by Messrs. Rickman and Thompson, encourages belief that this often attempted problem has at last approached a practical solution. The process is based upon the deoxidizing of air by coal dust in the presence of water vapor, at a full red heat, the water being simultaneously decomposed and yielding hydrogen, which combines in the proper proportion with the hydrogen thus set free. Common salt is introduced with the coal into the furnace wherein the reactions are effected, which, at the bright red heat maintained, is also decomposed. The free chlorine from the salt combines with the nascent ammonia, forming therewith ammonium chloride. The ammonia being thus combined, it does not decompose under the action of the heat, as would otherwise be the case. Should this process prove as practicable as has been anticipated, ammonium chloride will be furnished at a cheap rate and in large quantities, and will have great influence upon the future of artificial fertilizers.

The process invented by Mr. George Bower for coating iron with a preservative coating of magnetic oxide, appears to remove the difficulties attending the application of the method previously invented by Mr. Barff, who effected the same object by the use of super-heated steam, very difficult to manage, except in coating articles of moderate size. Mr. Bower effects the same object by burning carbonic oxide and air, in which the articles are placed.

The manufacture of paper from bagasse has attracted much attention and considerable experiment. A fiber sufficiently fine in texture to make excellent paper is obtainable from this refuse product, but it has not yet been successfully bleached. Doubtless this part of the manufacture, which does not appear to offer insurmountable difficulties, will, before long, be accomplished, and a valuable addition to paper-making materials will be supplied.

The manufacture of glucose has greatly increased during the year. As this substance has come to be largely used as an adulterant of sugar and confectionery, it is somewhat doubt-

ful that its increased use is a matter of congratulation. However, it is a thriving and growing industry. Old linen rags have been found available for the manufacture of glucose, and have been turned to account in Germany for the purpose. An establishment in that country is said to be making a ton of glucose per day from this kind of material.

Thus chemistry is daily, to a greater extent than ever before, affecting the industries of the world, supplying new articles of food, aiding land to produce larger crops, utilizing waste products, and teaching that, as in the economy of nature there is no waste, there should be none anywhere.

TREATMENT OF CANCER WITH CAUSTICS.

THE JUICE OF OXALIS ACETOSELLA AS A CAUSTIC.

By EDGAR ELTINGE, M.D., Brooklyn, N. Y.

THE *Oxalis acetosella*, familiarly known as wood-sorrel, is indigenous both to the United States and Europe, and is abundantly found in shady, waste places, groves and hedges, neglected or uncultivated grounds and unfrequented highways, and mountainous woody regions. Many other species having similar or like properties may be included under the same general head, containing the same remedial agent—the oxalate of potassa—to which it is believed to owe its acidity. It possesses marked caustic or escharotic properties, heretofore either unnoticed, untried, or unadmitted by the general profession, although it has formed the essential ingredient of the pastes used by some empirics.

The expressed, inspissated juice of this plant, properly formed into a suitable paste, I have successfully used as a local application in the removal of an epithelioma of the lip occurring in my own person. This had gradually and slowly enlarged, with alternate abrasions and partial healings, until it had become an inflamed, burning, painful, and offensively ulcerating tumor, attended with tumefaction of the sublingual, right parotid and right tonsillary glands, with copious salivary flow and an inordinate sebaceous secretion which was almost unendurably offensive. By none except the unfortunate possessor of so unfriendly and loathsome a body-companion can the abhorrence and foreboding with which it is accompanied be realized.

The best surgical advice recommended excision by the knife, but with an unfavorable prognosis.

My next trial was had with the zinc chloride paste of Canquoin, with which I had successfully operated several years before upon one whose ability to endure pain was commendable. But when used in my own case it aborted, on account of the excessive hemorrhage which its use provoked, a small portion only of the tumor being removed.

After a delay of two months, no especial good resulting, I was prompted to make a second trial of *Oxalis acetosella*. This second trial was made with perfect success.

Having some previous knowledge of the caustic effects of the juice of the oxalis, and desiring if possible to avoid the deformity as well as the uncertainty frequently attending excision, I was induced to give the inspissated juice a trial, but failed in the first application to effect the desired result, owing, as I subsequently found, to its too early withdrawal, my courage not being sufficient to cope with the pain self imposed. Three successive applications were made at intervals of twelve hours each, which were sufficient to entirely destroy the growth; the resulting eschar separated on the eighth day, leaving a healthy granulating surface, which rapidly healed. Not a drop of blood issued throughout, the contrast in this respect with the effect of the zinc paste being marked. The glandular tumefactions rapidly subsided, and now, at the end of two years, I still remain free from any signs of recurrence.

I found the pain produced by the application of this caustic to be intense, demanding all my fortitude to enable me to endure it. The duration, however, did not exceed half an hour after each application—a period during which it would be quite practicable to maintain general anæsthesia in patients requiring it.

THE ADVANTAGES OF CAUSTICS FOR THE REMOVAL OF MALIGNANT GROWTHS.

By FRANCIS H. STUART, M.D.

I AM glad the attention of the Anatomical and Surgical Society has been called to this subject, and that this mode of treatment has been so strikingly illustrated by the case of Dr. Eltinge, he himself being the patient. The use of caustics for the removal of morbid growths has been neglected by the profession in general, being looked upon as savoring of quackery. I suppose this is because persons ignorant of anatomy as well as of pathology, except in its dire results, have flourished as quacks—removing such growths with certainty, and often with good result, by means of caustic pastes. Dread of the knife is the universal feeling. We shrink from a mutilation of the body as well as shrink from the pain inflicted by the surgeon's knife. Happily the pain is not now to be endured. An anæsthetic certainly puts the sufferer beyond that, and also the sickening sight of the paraphernalia of the operating-room, so often unnecessarily displayed.

It is for these reasons that patients desert the offices of the studious and learned surgeons and flock to the houses of the "cancer doctor." The latter applies a "paste," which he hardly knows more of than its effects, having previously extorted a princely fee from his unfortunate victim. The diagnosis of the disease and the necessity for its removal has already been determined by the family surgeon(?). Perhaps the day has been fixed for an operation, to which the patient has looked forward with feelings scarcely less harrowed than if it were a day of execution. A friendly acquaintance tells of the bloodless and knifeless method of "Dr." ——, and thither the patient resorts.

I would not be understood as advocating the universal rejection of the knife and the adoption of caustics for such purposes. But I think that in caustic pastes we have a therapeutic, or rather surgical remedy that we could employ frequently, with as good or better results than are obtained from the knife. Over three years ago, while acting as adjunct surgeon to the Long Island College Hospital, I was consulted by a patient with cancer of the left mamma. Its removal was advised. The diagnosis and the remedy proposed was indorsed by other surgeons whom she consulted. She went to a "cancer doctor," who removed it with paste. I have since regularly visited the patient to observe the result. A better result, more thorough removal, and more symmetrical scar, I have never seen. About a year ago I was consulted by an elderly gentleman who had what was diagnosed by myself as commencing epithelioma on the right cheek over the zygomatic arch. The diagnosis was confirmed by the highest surgical authority of this city and of New York, and its immediate removal advised. Having in mind the above mentioned case, and also having recently had my attention called to the sulphate of zinc paste (Zinci sulph. ac. sulphuric. q. s. ut fiat magma), I determined to use it in this case, though the surgeon of New York, who was consulted, flatly denounced such a method. My patient was willing to submit to the treatment proposed. The face was shaved, and the surrounding surface protected by ointment. The paste was then applied, the cuticle having been destroyed by carbolic acid. It occasioned scarcely any pain at all. After three days the scar began to separate, and entirely fell off in seven, leaving a healthy-looking granulating surface, which healed in a surprisingly short time. The patient has since then had no further trouble.

The late Prof. Simpson was an enthusiastic advocate of sulphate of zinc as an escharotic. His statements in regard to its action are summarized by Dr. Stephen Smith, of New York, in an admirable lecture upon the subject, published in the New York *Medical Record*, February 14, 1880. It is as follows: "He states that the part to which it is applied is rapidly destroyed to a depth corresponding to the thickness of the superimposed layer; the slough is of a white color, and separates usually on the fifth or sixth day, leaving behind it, when the morbid tissue is removed, a red, granulating, healthy, and rapidly cicatrizing wound. This slough shows no tendency to chemical or putrefactive decomposition, but is firm in texture, and free from taint or odor; the local inflammatory reaction around a sulphate of zinc eschar is generally light and transient; there is no marked effusion or swelling in the surrounding parts, except where the caustic was used in the neighborhood of loose cellular tissue; the general system is not affected by its absorption, nor are there any constitutional symptoms, however freely and lavishly it is used."

Dr. Smith continues: 'We have in the sulphate of zinc a caustic which, in its various forms, is adapted to a great variety of conditions. Prof. Simpson sums up its advantages, as compared with other caustics, as follows: '1. Its powerful escharotic action; 2 The rapidity of its action; 3. Its great simplicity and manageableness; 4. Its facility of application; 5. Its non-tendency to deliquesce or spread; 6. Its perfect safety; 7. Its efficacy.' He speaks hesitatingly as to the seventh statement, but adds that he has seen not only the surface of cancroid and cancerous ulcers speedily and perfectly excavated by its application, but the surrounding characteristic induration become at the same time rapidly absorbed, and the remaining wound speedily cicatrizes. He has also found epithelial or cancroid ulcer of the cervix uteri, under the local application of the powdered sulphate of zinc, exfoliate its ulcerated surface, have its sanguineous and sero-purulent discharges arrested, the parts temporarily, if not permanently, healed, and the patient's health and strength and spirits restored; though, on first using the caustic, he believed the disease to be altogether beyond the reach of any remedial measure.

"My experience in the use of the sulphate of zinc as a caustic has been considerable, and fully coincides with that given above."—*Annals of Anatomy and Surgery.*

THE ANTISEPTIC TREATMENT OF TOOTH ROOTS.

THE attention of the profession having been of late greatly occupied in investigating the merits of antiseptic surgery, it may not be altogether unprofitable to consider how far the antiseptic principles are applicable to the teeth; and, if applicable, what the practical result of their application will be.

With this view, I have for the last year been employing two very powerful antiseptic agents in the treatment of dead roots, eucalyptus oil and iodoform, with very satisfactory results.

Eucalyptus oil and iodoform are both very much more powerful antiseptics than carbolic acid, and their effects are much more permanent.

When a tooth dies, partially or wholly, the slough which remains differs in no respect from a slough in any other part of the body; if it become septic (as in the natural course of events it must), it will cause inflammation and suppuration, and be rejected from the living economy. Furthermore, if these products of inflammation be prevented from discharging themselves through the open cavity, owing to presence of a filling, they will collect inside and force their way up the fang, and end by causing an alveolar abscess. Now, if a local slough in any other part of the body be rendered and kept perfectly aseptic, the behavior of the parts is very different; there is neither *irritation* nor *inflammation*; the dead tissue, remaining perfectly white, pure, and sweet smelling, is gradually removed by absorption, not rejected as discharge; the temperature does not rise, there is no pus; the slough is quietly absorbed and replaced by new, living, cicatricial connective tissue. This result may be seen at any time in Professor Lister's hospital practice or anywhere else, where the antiseptic precautions are punctiliously observed.

If then this asepticity be produced in the pulp cavity of a necrosed tooth, that is, if all the bacteria it contains be destroyed, and the entrance of fresh ones prevented, then the slough it contains, whether it be part or all of the pulp, may be sealed up with an impermeable filling, without fear of any of the unpleasant sequelæ above alluded to. The slough will be removed not by inflammation and suppuration, but by absorption and replacement by cicatricial tissue. There will be no abscess, because there will be no inflammation, the dead pulp having been rendered inert.

From theory to turn to practice, the whole gist of the matter lies in the words *perfectly aseptic;* antiseptics to be any use at all must be carefully and thoroughly applied.

The method of procedure I have adopted has been very simple, and hitherto invariably successful. It has simply consisted in clearing the cavity, removing a little of the slough from the pulp cavity, and dressing the tooth with eucalyptus oil and iodoform on a piece of wool every day (the patient can do this for himself, if necessary), until no smell but that of eucalyptus oil can be detected when the dressing is removed. It may then be sealed up with gutta percha, as an experiment (the eucalyptus has a slight solvent action on gutta percha, which renders the stopping very perfect). It is perfectly aseptic; there will be no disturbance; and the tooth, after three weeks or so, may be permanently filled. If the aseptic condition is not perfect, the patient will soon experience unpleasant sensations, in which case the gutta percha must, of course, be taken out and the dressing resumed, and the gutta percha tried again after another interval. Of course, all pus and debris should be removed first with an excavator.

The smell and taste of eucalyptus oil is rather pleasant, and it causes no irritation whatever to gum or lips, and this is no unimportant point to a patient who has to use the dressing for a long time, especially if he has had any previous experience of creosote.

Under these circumstances, if a pulp be partially dead, or if one root be dead and the other living, it is not necessary to destroy the living part with arsenic.

Lastly, in the case of an alveolar abscess, the effect of eucalyptus oil when injected is more rapid and effectual than that of any other agent with which I am acquainted, because its antiseptic powers are not only greater but more *permanent*, which latter is a very important point in regions where bacteria teem as they do in the mouth.

The power of iodoform to avert suppuration has been much testified to recently in all the medical papers, and the rapidity with which I myself have seen chronic ulcers and soft chancres disappear under it is almost magical. Eucalyptus oil dissolves iodoform, so that they can be easily used together.—*Underwood, in Monthly Review of Dental Surgery.*

CHARCOAL IN INFANTILE DIARRHEA.

BEFORE the Paris Academy of Medicine Dr. Guerin stated that a teaspoonful of powdered charcoal in a cup of milk with a little coffee added was excellent treatment. In a little while—sometimes the first day—the passages change consistence and odor and the green color becomes natural. This mixture is easily made, and infants drink it as if it were milk alone. At the same time Dr. Guerin adds a little sweetened water to one-third or one-half milk, and infants like it, and often it stays vomiting. He has seen infants sick six or eight days with uncontrollable diarrhea recover in three days.

A CATALOGUE, containing brief notices of many important scientific papers heretofore published in the SUPPLEMENT, may be had gratis at this office.

THE
Scientific American Supplement.
PUBLISHED WEEKLY.
Terms of Subscription, $5 a Year.

Sent by mail, postage prepaid, to subscribers in any part of the United States or Canada. Six dollars a year, sent, prepaid, to any foreign country.

All the back numbers of THE SUPPLEMENT, from the commencement, January 1, 1876, can be had. Price 10 cents each.

All the back volumes of THE SUPPLEMENT can likewise be supplied. Two volumes are issued yearly. Price of each volume, $2.50, stitched in paper, or $3.50, bound in stiff covers.

COMBINED RATES.—One copy of SCIENTIFIC AMERICAN and one copy of SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, $7.00.

A liberal discount to booksellers, news agents, and canvassers.

MUNN & CO., Publishers,
37 Park Row, New York, N. Y.

TABLE OF CONTENTS.

PAGE

I. ENGINEERING AND MECHANICS.—Calvert Street Bridge, Baltimore. Full page illustration. Perspective, elevations, sections, details..... 4837
Do Stays Stay? Communication from A. HERBERT CRAWFORD. 4838
Improved Water Gauge for Boilers. 3 figures..... 4838
Device for Cleaning Boiler Tubes. 1 figure..... 4838
A Newly Discovered Property of the Ellipse, and its Application to the "Oval Chuck." By FRANK M. LEAVITT. 2 figures..... 4838
The Lighthouse of Ar-men, Finisterre. 4 figures. Elevation, vertical section, plan. Elevation and plan of the rocks at Ar-men. 4839
Failure of a Dock Wall. By Lieut. H. D. LAFFAN, R.E..... 4839
Approximate Rules for the Penetration of Armor..... 4840
Short Lessons to Shipbuilders..... 4840
Experiments on the Position of Screw Propellers. 3 figures..... 4841
United States Ocean Commerce in United States Built Ships. 4841
National Convention Paper, by W. H. WEBB..... 4841
Plain and Ornamental Borders for Coach Painting. By THEO. CHILD. 14 figures..... 4842
A Librarian's Notion of What a Library Building should be. W. F. Poole's paper before the Convention of the American Library Association..... 4842
House Drainage, Sewerage, and Ventilation. Paper read by Reginald Middleton before the Society of Civil and Mechanical Engineers, London..... 4843

II. TECHNOLOGY.—Every Man his own Sensitive Plate Maker. By J. A. FORREST. Gelatine Dry Plates.—Substratum.—The Emulsion.—To Wash the Emulsion.—Coating and Drying Plates.—Development.—Intensifying.—Supplant Plate Drying Box..... 4844
On Rational Seasoning of Wood, with special regard to the "Beerizing" Process. By SIGISMUND BEER. What wood really is.—A simple experiment.—Explanation.—Creosoting lumber.—The "Beerizing" process.—Its cheapness and quickness..... 4845
Sundry Notes: Use of Sirups in Brewing.—The Influence of Oxygen on Different Ferments.—Researches on an Acidifying Ferment.—Beer in Frosty Weather.—A Process for the Manufacture of Ammonia.—The Barff Process of Coating Iron.—Paper from Bagasse.—Glucose Manufacture..... 4846

III. MEDICINE AND HYGIENE.—Arsenical Wall Paper and Artificial Flowers. Serious cases of poisoning thereby..... 4846
Treatment of Cancer with Caustics. The juice of oxalis acetosella as a caustic. By Dr. EDGAR ELTINGE..... 4847
The Advantage of Caustics for the Removal of Malignant Growths. By Dr. FRANCIS H. STUART..... 4847
Antiseptic Treatment of Tooth Roots..... 4847
Charcoal in Infantile Diarrhea..... 4848

IV. PHYSICS.—Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter. By JOHN TYNDALL. Two recent and highly important communications to the Royal Society..... 4848
On Some Physical Phenomena. By W. J. MILLAR. Force.—Matter.—Motion.—Space.—Time.—Velocity.—Momentum.—Energy.... 4849
Sir William Thomson's Harmonic Analyzer. 5 figures..... 4850
The Jordan Barometer..... 4850
Glass as an Obstructor of Light..... 4850
A Simple Transmission Dynamometer. By ELIHU THOMSON. 2 figures..... 4850

V. CHEMISTRY.—Chemical Decomposition Incited by a Cold Fluid Stratum Floating on a Warm Liquid. By Dr. HENRY A. MOTT, Jr. 4850
Upon the Production of Ozone by Heating Substances Containing Oxygen. By Dr. A. R. LEEDS..... 4851
Separation of Cobalt and Nickel. By F. REICHEL..... 4851

VI. NATURAL HISTORY.—On the Grains of Silica and Micrococci of the Atmosphere. By Dr. T. L. PHIPSON..... 4851

VII. ART.—Suggestions in Decorative Art. Panel from the Communion Table in the Cathedral of Verdun. Designed by A. G. Moreau, Paris..... 4851

PATENTS.

In connection with the **Scientific American,** MESSRS. MUNN & CO. are Solicitors of American and Foreign Patents, have had 35 years' experience, and now have the largest establishment in the world. Patents are obtained on the best terms.

A special notice is made in the **Scientific American** of all Inventions patented through this Agency, with the name and residence of the Patentee. By the immense circulation thus given, public attention is directed to the merits of the new patent, and sales or introduction often easily effected.

Any person who has made a new discovery or invention can ascertain, free of charge, whether a patent can probably be obtained, by writing to MUNN & Co.

We also send free our Hand Book about the Patent Laws, Patents, Caveats, Trade Marks, their costs, and how procured, with hints for procuring advances on inventions. Address

MUNN & CO., 37 Park Row, New York.
Branch Office, cor. F and 7th Sts., Washington, D. C.